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THE NATURE AND ORIGIN OF LIVING MATTER

BY

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"To experience we refer, as the only ground of all physical inquiry. But before experience itself can be used with advantage, there is one preliminary step to make, which depends wholly on ourselves : it is the absolute dismissal and clearing of the mind of all prejudice from whatsoever source arising, and the determination to stand or fall by the result of a direct appeal to facts in the first instance, and of strict logical deduction from them afterwards."

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NOTE

The Illustrations pertain to Chapters IX–XIII, and the descriptions of the several Figures will be found in the text in the immediate neighbourhood of the Plates.

CHAPTER I

THE CORRELATION OF VITAL AND PHYSICAL FORCES

NEAR the close of the eighteenth century (1798), Benjamin Thomson, afterwards Count Rumford, announced to the Royal Society his conviction, based upon experimental evidence, that "heat was a mode of motion." This conception was the germ of the important doctrine known as "The Correlation of the Physical Forces" enunciated in 1842 by Grove in this country and almost simultaneously by Meyer a physician at Heilbronn—a doctrine according to which heat, light, electricity, magnetism and chemical affinity (the so-called imponderables) were at once convertible and indestructible—and all capable of arising from or giving origin to motion either directly or indirectly. In succeeding years this doctrine was developed by many other workers, the most prominent of them being Joule, Clausius, Rankine, Thomson (now Lord Kelvin) and Helmholtz.

The speculations of ancient philosophers, such as Democritus, Epicurus, Lucretius, and others to the effect that life could be resolved into a series of physico-chemical phenomena, now began to be looked at from a more positive basis. The first real step in this direction was taken in 1845 by Meyer of Heilbronn in a memoir on "Organic Movement in Relation to Material Changes," in which he showed that the processes taking place in living organisms, animal or vegetal, were produced by forces acting upon them from without, and that the changes in their composition brought about by these external agencies were the immediate sources of those modes of force apparently generated in the organisms themselves. A few years later this view was elaborated by Dr. Carpenter in a memoir published in the Philosophical Transactions, "On the Mutual Relations of the Vital and Physical Forces," in which he sought to show "that so close a mutual relationship exists between all the vital forces, that they may be legitimately regarded as modes of one and the same force." He

maintained that these so-called vital forces were evolved within the living bodies of plants and of the lower animals by the transformation of the light, heat and chemical action obtained from without; which forces were given back to the external world again, either during the life of the living beings or after their death in terms of motion and heat, and also, to a slight extent, in the form of light and electricity. This has been found to hold good, as we shall see later on, for plants and the lowest animals and also for the initial changes in higher animals, though all the later vital manifestations of the latter are dependent almost entirely upon the redistribution of the forces locked up in the organic substances which constitute their food, and to the various chemical changes taking place within their bodies at the expense of their own tissues.

We have thus to do with a fundamental distinction between the nutritive processes of plants and animals, for, as pointed out by Gavarret in his interesting work "Phénomènes Physiques de la Vie" (1869), most of the physical force which, in the form of light and heat, impinges upon a plant, is consumed therein (*travail intérieur*). It is stored up as potential force in the complex organic substances entering into the composition of the plant; these organic substances being produced (under the influence of the already existing living tissues) by the action of the above-mentioned physical forces upon the not-living constituents of the earth, air, and water by which the plant is surrounded. The animal, on the contrary, liberating and using the forces which have been stored up by the plant—after assimilating a part of its substance in the form of food—expends them in the production of that *travail extérieur* which the animal's nature and the necessities of its existence compel it to manifest.

Now, as we all know, animals display, in varying proportions, three principal modes of 'vital' activity which testify to the continual liberation of force within them:—(1) they appear to produce heat; (2) they move, by reason of the contractility of certain tissues; and (3) they display certain nervous phenomena. Let us briefly consider each of these manifestations.

1. Very many animals constantly maintain themselves at a temperature above that of the medium in which they live; this being more especially the case with the so-called 'warm-blooded' animals—amongst which birds are most remarkable for the very great difference existing between their temperature and that of

the air. The cause of this difference in temperature between the animal and its medium has been variously explained at different times. The true theories on this subject, however, may be said to date as far back as the close of the eighteenth century, and to have commenced with the brilliant discoveries of Lavoisier. He showed that the oxygen introduced by the respiratory passages attacks the organic substances furnished by digestion, burns them, combining with their carbon and their hydrogen to form carbonic acid and water. He showed that this slow combustion of the organic materials of the blood is an incessant source of heat. Lavoisier then instituted experiments to determine the quantity of heat abstracted from the animal by radiation, by contact with air, and by evaporation of fluids from the surface of the body. On the other hand, he measured the quantity of oxygen consumed, calculated the proportions of carbonic acid and of water produced by the combination of this oxygen with the materials of the blood, and then estimated the quantity of heat disengaged during these reactions. From a comparison of the results thus obtained in these two series of observations, he came to the conclusion that the chemical reactions carried on within the body would furnish enough heat to maintain the animal at its proper temperature. This conclusion was afterwards confirmed by many other experiments and observations. The researches of Lavoisier still left us in doubt, however, as to whether the combustion of the materials of the blood took place in the capillaries of the body generally, or in those of the pulmonary circulation. This doubt was removed by Spallanzani; and the subsequent experiments of Magnus and of Claude-Bernard only tended to confirm his conclusion, that the heat-producing chemical changes were carried on in the capillaries of the body generally. Thus the heat evolved in animals is some of that solar heat which had previously impinged upon plants, and which was gradually locked up in the form of potential energy during the growth of the plant-tissue, subsequently taken as food by animals.

2. Turning now to the next dynamic manifestation of animals—to their power of movement—we may, for the sake of brevity, consider this as it presents itself in the higher animals only—in those in which the movements depend upon the contractility of definite structures known as 'muscles.' *Contractility* is the essential attribute of the muscle, and, being one of the peculiarly vital endowments, we may now enquire how far this vital property is one which is

correlatable with ordinary physical forces, or whether it can display itself independently of these.[1]

After discussing the many investigations which have been made on this subject Gavarret summarised their results as follows:—"All these experiments agree in showing that in the muscular system of an animal which accomplishes *actual work* (such as raising a weight, dragging a load, etc.) everything goes on as in an ordinary steam-engine. While the muscle *performs work*, the heat produced by the internal combustion becomes divided into two complementary portions; the one part appears as *sensible* heat, and determines the temperature of the muscle, the other *disappears*, so far as its existence in the form of heat is concerned, and, by the intervention of the muscular contraction, *becomes transformed into mechanical work*. The muscle is an *animated machine*, which, like the steam-engine, utilises the heat in order to produce work: in both cases there is necessarily an *equivalence* between the heat which disappears, or is consumed, and the external work achieved."

In consequence of its origin, the energy manifested during the contraction of the muscle is directly comparable with the energy due to the elasticity of vapour when this is the motor power at work, as in a steam-engine. Chemical change—combustion, in fact—in each case, in muscle and in steam-engine alike, causes the liberation of heat; and in each case part of this liberated force is capable of manifesting itself anew in the form of mechanical energy. It matters not whence the heat is derived—whether it comes from the decomposition of the recently assimilated food-products, in the blood which circulates through the muscle, or whether it proceeds from the liberated energy or sun-force that may have been locked up for ages in the bowels of the earth, but which is now set free by a process of combustion in the engine fire—the result is the same, and in the muscle, as much as in the steam-engine, we have to do with a machine in which the transference of heat into mechanical energy is capable of being effected.

The muscle, it is true, is a much more subtle kind of machine, and the precise mode of its action is as yet hidden from us; we know not *how* it is—through what precise molecular changes taking place in the substance of the muscle—that the heat which disappears as heat, is, through the property of *contractility*, enabled to reappear in the form of mechanical energy while the animal

[1] For a full and admirable treatment of this question we must refer the reader to pp. 120–194 of the work of Gavarret, already quoted.

performs its manifold muscular movements. That this is so, however, we know; and we know, moreover, that as a mere machine for the conversion of heat into mechanical energy, the muscle far excels the best steam-engine which has ever been constructed.

3. Turning now to the third mode of vital activity—to that which manifests itself in the display of nervous phenomena—we find that these manifestations are also closely dependent upon the integrity of certain material structures, and that their appearance coincides with an increase in the quantity of heat appreciable in, or in the neighbourhood of, these structures.

It has been ascertained very definitely by the experiments of Helmholtz and of Schiff, that the transmission of a stimulus through a nerve is marked by a rise of temperature therein; while the extremely interesting experiments of Lombard seem to show that a similar though minute rise of temperature takes place in the brain itself when it is in a state of activity.

The molecular motion or energy, set free in the nervous system, subserves very different purposes. Upon evidence which cannot now be gone into, it could be shown that the nervous system plays an important part (*a*) in regulating the various secretions and in influencing the nutrition of the body generally. It is nerve-force again (*b*) which initiates or calls into play the activity of the various muscles by which the countless movements within the bodies of animals are produced, and also those by which locomotion and external visible movements generally are effected. But nerve-changes also (*c*) give rise to other manifestations—manifestations altogether peculiar in kind and peculiar to the individual in whom they occur. Feeling is the basis of 'Consciousness,' and of 'Intellectual Actions' and these are phenomena which are commonly believed to be called into existence by the activity and the occurrence of molecular changes within certain parts of the nervous system and of the brain generally.

From a consideration of these different modes of activity of the nervous system it may easily be imagined how hopeless the attempt would be to establish anything like a quantitive estimate of the amount of force answering to the above-mentioned different results of nervous action. In considering the question of muscular activity and its correlation with physical force, we have to do with a measurable effect under the form of mechanical energy. But the manifestations of the activity of the nervous system are much more

subtle and eluding. How is it possible for us to estimate the value of the energy expended in regulating the nutrition of the body? How, in a motor act, shall we separate what is due to the nerve and what to the muscle? Nay, where Feeling is aroused, where Consciousness appears, how shall we estimate the equivalent value of this, which each one knows in himself alone, and which seems to differ so absolutely from everything else in the universe? However probable it may be that what we know as 'Sensation' and 'Thought' are as truly the direct results of the molecular activity of certain nerve-centres, as mechanical energy is the direct result of a muscle, this cannot be proved. Still, conscious states or feelings are admitted to be an appanage only of nerve actions; and their mode of appearance, their increase in intensity, and their modifiability by agents modifying the nerve tissues, together with the limitation by which they occur only in association with certain nerve actions taking place in the highest and most complex of an animal's Nerve Centres—all these facts harmonise with the notion that they are in some way an actual outcome of such Nerve Actions, no more capable of being dissevered from the physical conditions on which they depend, than is Heat to be dissevered from its physical conditions. Matter and force are inseparable—neither can exist alone. But to show how these particular motions in Nerve Tissue arise which underlie Conscious States, and how they again subside into mere ordinary nerve actions, must, from the very nature of the problem, ever remain insoluble.

Having briefly glanced at these higher manifestations of life taking place in the bodies of animals, and having indicated how intimate are the relations existing between such higher manifestations and the physico-chemical processes ever going on in their various organs during the assimilation of food, we must now, in our attempt to form an estimate of the nature of life and of living matter, look at the simpler problems presented by plant life.

Plants, in fact, are the active agents ever ministering to the wants of animals. They, in fashioning their own structures, are continually giving birth to organic substances which are to constitute the materials necessary for the maintenance of animal life. Thus Dumas in an interesting little work by himself and Boussingault on "The Chemical and Physiological Balance of Organic Nature" (1844) says :—

"M. Boussingault has demonstrated that plants in full growth

always take carbon from the carbonic acid of the air, hydrogen from the water which bathes them, and frequently azote from the air. . . . The soil he used for the growth of his plants, the subjects of experiment, was a siliceous sand, which was first sifted, then kept at a red heat for some time, in order to destroy every trace of organic matter within it. It was then moistened with distilled water, and the seeds sown; after an interval of a few days, the seeds which did not germinate were removed. . . . Peas planted in a soil absolutely barren, and watered with pure water, may attain to complete maturity, passing through all the phases of their natural growth, and bearing flowers and ripe seeds. During this process, they fix a large quantity of azote, which they must derive either from the air dissolved in the water which they absorb by their roots, or from the air that surrounds their stalks and leaves."

Animals, as a rule, are powerless for the creation of organic matter; they can assimilate and modify the organic substances which have been built up for them in the tissues of plants; but they cannot abstract from earth, air, and water the elementary constituents of organic matter, and force them to enter into such and such combinations. They use the materials which have been elaborated for them by plants, since they all feed either directly upon members of the vegetable kingdom, or else indirectly by living upon animals which have been so nourished. Plants, then, are the great factors of organic matter—the vegetable kingdom is nature's laboratory, within whose sacred precincts not-living matter is coerced into more elevated and complex modes of being, and is made to display those more subtle characteristics which we find in living tissues. Using only the great forces of nature—availing themselves only of the subtle motions emanating from the Sun under the names of heat, light, and actinism—plants compel carbonic acid to yield up its carbon, water its hydrogen, and nitrate of ammonia its nitrogen.

It might be thought that plants derive the principal part of the ingredients with which they build up their own structures from the soil; but the experiments of Boussingault just referred to have long since disproved this formerly favoured assumption, and shown that the atmosphere is the great storehouse for the pabulum of plants. Carbon is the most fundamental ingredient of the vegetable kingdom; all plants fix this substance, and all obtain it from carbonic acid—either abstracting it directly from

the air by their leaves, or obtaining it through their rootlets. In the latter case they may obtain it from rains which have fallen to the earth impregnated with the carbonic acid of the atmosphere, or else they procure it from that which is liberated by the gradual decomposition of organic particles in the soil. But that the air is the great storehouse whence, either mediately or immediately, plants procure their carbon, is rendered more and more obvious to us by the consideration of such facts as those to which Schleiden refers in his "Biography of a Plant" when he says:—"From forests maintained in good condition we annually obtain about 4,000 lbs. of dry wood per acre, which contains about 1,000 lbs. of carbon. But we do not manure the soil of the forests, and its supply of humus, far from being exhausted, increases considerably from year to year, owing to the breakage by wind and the fall of the leaf. The haymaker of Switzerland and the Tyrol mows his definite amount of grass every year on the Alps, inaccessible to cattle, and gives not back the smallest quantity of organic substance to the soil. Whence comes this hay if not from the atmosphere? The plant requires carbon and nitrogen, and in the woods and on the wild Alps there is no possibility of its acquiring these matters save from the ammonia and carbonic acid of the atmosphere."

As Dumas says:—"It is in plants, consequently, that the true laboratory of organic nature resides; carbon, hydrogen, ammonium, and water are the elements they work upon; and woody fibre, starch, gums, and sugars, on the one hand, fibrine, albumen, caseum, and gluten, on the other, are the products that present themselves as fundamental in either organic kingdom of nature—products, however, which are *formed in plants, and in plants only, and merely transferred by digestion to the bodies of animals.*"

Such, then, is the mighty round of things, such are the interchanges ever taking place on the surface of our globe. The inorganic is continually being fashioned into the organic, and this after passing through successive changes, and after having displayed the manifestations of Life, is ever passing again into the inorganic, ever again giving up its fashioning forces. "The crude and formless mass of the air gradually organised in vegetables, passes without change into animals, and becomes the instrument of sensation and thought; then vanquished by this effort, and, as it were, broken, it returns as crude matter to the source whence

it had come." "Thus," Dumas also says, "is the mysterious circle of organic life upon the surface of the globe completed and maintained! The air contains or engenders the oxidised substances required—carbonic acid, water, nitric acid, and ammonia. Vegetables, true reducing apparatus, seize upon the radicals of these, carbon, hydrogen, azote, ammonium; and with them they fashion all the variety of organic or organisable matters which they supply to animals. Animals, again, true apparatuses of combustion, reproduce from them carbonic acid, water, oxide of ammonium, and azotic or nitric acid, which return to the air to reproduce the same phenomena to the end of time."

Thus it would appear that the most simple not-living or mineral constituents coming into relation with one another in the presence of pre-existing protoplasm, appear, for aught we know to the contrary, to fall at once into those subtle combinations which constitute the basis of 'living' matter. The rapidity of the process mocks and defies all theoretical explanation. Here, at all events, there seems to be no laborious process of synthesis—no long chain of substitution compounds—before the final product is evolved.

But the property of decomposing ammonia and of feeding upon elementary mineral substances is by no means confined to the higher plants. The same power is possessed by Conferva vulgaris and other low Algæ, as was demonstrated by Bineau more than fifty years ago[1]; while nearly ten years afterwards it was ascertained by Pasteur that some of the lowest kinds of fungi, the Mucedineæ, were capable of growing and multiplying in a solution of sugar and tartrate of ammonia, to which a trace of some phosphate had been added. Referring to Pasteur's observations, Baron Liebig said:—"It is astonishing that this discovery has not attracted more attention in regard to a special point, for it comprises a fact of very great significance for physiology, viz., the formation of albuminates in plants, respecting which we are in possession of scarcely anything beyond conjecture; hitherto this has been regarded as one of the greatest mysteries of organic nature. . . . If yeast cells, placed in a mixture of ammonia, tartaric acid, sugar, and phosphate, could propagate and multiply, it is evident that an albuminate must have been formed from the

[1] Mém. de l'Acad. des Sciences de Lyon, t. iii, 1853.

elements of this mixture, since one of the chief constituents of the yeast fungus is an albuminoid substance."

All that is here said by Liebig becomes even still more striking after my own observations, as to the freedom with which Bacteria and Torulæ multiply not only in solutions of ammonic tartrate to which a phosphate has been added, but also in solutions of tartrate of ammonia alone.[1] The fact that this occurs shows that these simple saline substances not only contain the elements necessary for the formation of living matter, but that the passage must be comparatively easy from the saline mode of collocation of the elements into that by which they are converted into living protoplasm. Nay, more, seeing that the multiplication of living things takes place with so much more energy and rapidity in a solution of ammonic tartrate than it does in one of the oxalate, the acetate, or even the carbonate, it seems to show that the ammonic tartrate state of combination is an especially favourable platform for the initiation of these new and more complex modes of combination.

This fact, that growth and multiplication of elementary living organisms takes place at the expense of the elements of the saline solution, shows that under a certain influence—that of the pre-existing living matter added thereto—the elements of the saline solution are capable of reacting in such a manner as to fall into new modes of combination, whereby they give rise to 'living' compounds. But if, as we contend, no special or peculiar forces are at work within the pre-existing organisms, the molecular movements constituting their 'life' must be determined purely by natural affinities, so that they can only exert some catalytic action which is essentially physico-chemical upon the molecules of the matter with which they are brought into contact. If, then, under the influence of these chemical actions the molecules of the saline substances and of the water in which they are dissolved undergo a rearrangement and combination whereby they are converted into living protoplasm, we are compelled to assume the truth of what appears to be on other grounds so probable, that there is a natural aptitude for the molecules of certain compounds to fall into the more complex modes of combination that exist in living matter.

In regard to the general doctrine of "The Correlation of the

[1] "The Beginnings of Life," 1872, Vol. ii, *Appendix*, pp. xlvi–liii.

Vital and Physical Forces" it must be borne in mind that when a physical force gives rise to any one of the modes of vital force, what takes place is not so much a direct conversion or transmutation of the force itself, but rather that the physical force expends itself in bringing about new collocations of matter—either in converting not-living into living matter, or in altering the molecular constitution of matter which is already alive. The properties of this matter being what we call 'vital,' it may be said that the physical force has been transmuted into vital force. Only when understood in this sense, are the words 'conversion' or 'transmutation' suitable for the expression of what really occurs. The almost necessary use of these terms has nevertheless tended to foster an erroneous impression, which has exercised its misleading influence by causing certain physiologists to suppose that a special 'vital force' is needed to effect the transmutation of incident physical forces within the bodies of living organisms. In reality, no special force is in the least needed. Any pre-existing physical force, acting upon an organism, expends itself in producing those molecular rearrangements which, with others, contribute to enable the organism to carry on its so-called 'vital' processes. If the doctrine of the Correlation of the Vital and of the Physical Forces is admitted to be true, it can, we think, be accepted only in this form, and the vitalists must give up their last stronghold—we cannot even grant them a right to assume the existence of a special 'vital force' whose peculiar office it is to effect the transformation of physical forces. The notion that such a force exists is based upon no evidence; it is a mere postulate—an appanage of obsolete views. The assumption of its existence carries with it nothing but confusion and is totally adverse to the general doctrine of the Correlation of the Forces. Need we say more? Does it not follow that if living units of the simplest kind are ever now evolved in solutions containing organic matter, such rudimentary forms of life are to be regarded as resulting from the collocations of organic molecules in peculiar modes, brought about by the expenditure of incident physical forces?[1]

[1] Living matter, like crystalline matter, is only formable by a synthesis of its elements. As Crystals have not the power of self-multiplication they have only one mode of production. But because Organisms have reproductive powers, the obviousness of these modes of increase has sufficed to cast doubts upon the reality of the independent origin of living units.

The plant, as we have seen, takes not-living mineral ingredients from earth and air, and under the influence of solar light and heat, these simple materials assume, within its tissues, those higher modes of combination which are necessary in order that they may be converted into 'living' matter. The forces at work in and upon the plant are supposed to be nothing more than ordinary physical forces. Here, however, it is true, matter passes from the lifeless to the living state of combination under the influence of pre-existing protoplasm. But as no unknown and independent forces are now believed to be at work within living tissues we must suppose that, under the influence of the physical forces within and those without the organism, lifeless compounds fall more or less immediately into such and such living combinations. The influence of the pre-existing protoplasm would seem to expend itself chiefly in the building up of the living matter after its own likeness. Here, at all events, facts lend no support to the common notion that there is some extraordinary difficulty to be overcome in order that matter may fall into living modes of combination. The further common supposition as to the necessity for long intervals of time, during which numerous intermediate stages in molecular complexity may be passed through before the combinations resulting in what we know as living matter can arise, seems directly opposed to the facts made known concerning the nutrition of plants and the free growth of Bacteria and Torulæ in comparatively simple solutions of not-living matter.

CHAPTER II

THE FUNDAMENTAL PROPERTIES OF LIVING MATTER

SEVERAL causes tend to augment the mystery inseparable from our thoughts concerning living matter. One of these is due to the common notion of the existence of a separate entity or vital principle, called 'life'—a notion which, strangely enough, has been fostered by many celebrated physiologists and philosophers such as Bichat, De Blainville, Richerand, Schelling, and even Herbert Spencer, by their attempts to find a definition of 'life.'

But all things possessing qualities—that is everything in the universe—has a 'life' of its own, varying though it may in rank and relative supremacy. All bodies in nature are in fact known to us only as aggregates of such and such properties. They are, however, divided into two great classes—the living and the not-living—according as they do or do not possess a small group of certain qualities. These differentiating qualities are those which are generalised and included under the abstract name 'life.' We must not be blinded, however, by the use of such a word; we must not fall into the old error of supposing that because by a process of generalisation we have conceived a mere abstract notion to which we attach the name 'life,' that there is anything existing, of and by itself, answering to this term. No, each material body has properties of its own—properties which are due to its molecular constitution—and which make it what we know it to be. Certain of the objects around us, for instance, have a power of assimilation, of growing, of developing, and of reproducing their kind. Bodies possessing such properties have been arbitrarily named 'living' bodies, and the word 'life' has been used as a mental symbol connoting the sum total of the properties which distinguish such bodies from the members of the other great class whose representatives do not possess them. These properties are undoubtedly of a much higher and more subtle nature than those of not-living matter, but it should be distinctly understood that

they are as much dependent upon the mere qualities and nature of the material aggregate which displays them, as the properties of a metal or the properties of a crystal are the results of the nature and mode of collocation of the atoms of which such bodies are composed.

Putting it still more plainly we may say that the phenomena manifested by living things are dependent upon the properties and molecular activities of a particular kind of matter known as protoplasm, just as mental phenomena are dependent upon the properties and molecular activities of nerve-tissues, and just as magnetic phenomena are dependent upon the properties and molecular activities of certain kinds or states of iron. And though generalised conceptions of these several kinds of phenomena have been embodied in the terms 'life,' 'mind,' and 'magnetism' respectively—neither of which corresponds with any independent existence—yet regarded as ultimate facts, we are just as impotent to 'explain' the relation, or nexus of causation, existing between magnetic phenomena and the one set of molecular activities, as we are to 'explain' the relation between the phenomena presented by the simplest forms of 'life' and the molecular constitution and activities of protoplasm.

As the writer pointed out long ago[1] the attempted definitions of 'life' that have been made have been so many generalised statements, more or less distinctive, concerning the phenomena presented by living things, one of the best of which is that given by Herbert Spencer as "The continuous adjustment of internal relations to external relations." On the other hand according to Schelling, life is the "principle of individuation, or the power which unites a given all into a whole." However unsatisfactory this may be as a definition of life, we cannot fail to recognise that it is an expression of one of the most notable tendencies revealed in all the higher forms of life.

Philosophically speaking there can be no abrupt line of demarcation between the living and the not-living, living things being only peculiar aggregates of ordinary matter and of ordinary force, whose constituents, in their separate states, are not possessed of the qualities connoted by the word 'life.' *Omnia mutantur: nihil interit.* As Dumas has said,[2] "there is an eternal round in which

[1] "The Beginnings of Life," 1872, Vol. i, p. 71.

[2] "The Chemical and Physiological Balance of Organic Nature," 1844, (Transln.) p. 48.

death is quickened and Life appears, but in which matter merely changes its place and form."

Another cause tending to complicate popular conceptions concerning 'life' is what we may call an anthropomorphic fallacy—a tendency, that is, to endow living things with such attributes as we may recognise in ourselves, or at least in higher members of the animal kingdom generally. The more we look to the higher forms of life the more apt are we to be blinded as to the real and essential nature of the phenomena taking place, owing to the great complexity which has arisen in the various functions of these higher forms step by step with the increasing complicacy of their organisation.

The more highly developed an organism has become, the more has specialisation been brought about in the functions of its several parts, and (in almost the same proportion) the more, as Schelling has said, has the *all* become welded into a *whole*. The greater the degree of interdependence existing between the actions of its several parts, the more is the well-being of the entire organism interfered with by damage occurring to any one of these principal parts. Through the intervention, for the most part, of the nervous system and the vascular system, this individuality of the entire organism is carried to the most marked extent in the highest vertebrata, so that the 'life' of one of these creatures—regarded as a whole, or sum total of phenomena—differs almost as widely as it is possible from that of some of the lowest animals on the one hand, and from that of plants on the other. Their mode of death also is quite different. And as with 'life,' so is it with 'death,' we are perhaps too apt to form our notions concerning each from what we see taking place in man himself and in the higher living things.

Many people apparently never reflect upon the striking differences which are presented, in this respect, by the lowest animals as well as by the members of the vegetable kingdom. In man we find a fully developed and excessively complex organisation, in the working of which, as in that of any ordinary but extremely complex piece of machinery, there is seen to be the closest interdependence between the actions of the several parts. The action of some parts are more essential, that of others less essential to the action of the machine as a whole. An interference with the revolution of some central wheel may suffice instantly to

interrupt the working of the entire mechanism, just as the functional workings in the body of a highly organised vertebrate animal may be as suddenly arrested by a puncture in a particular part of its nervous system. In both instances the first result is a simple cessation in the action of a complex machine ; and, in the case of the animal—seeing that its body has been gradually built up in a given manner under the influence of certain definite actions or 'functions,' the continuance of which is absolutely necessary—it follows that when such actions are arrested irretrievably, the organism as an individual whole must die, although its separate parts and anatomical elements may and do perish much more slowly, after different intervals.

If the medulla oblongata has been punctured and the heart has ceased to beat, there is a permanent stoppage of this function, without which 'life,' in such a being as a mammalian vertebrate, is impossible. It consequently dies. If the blood no longer circulates, the anatomical elements, which are absolutely dependent upon this fluid for their pabulum, must also, after a time, necessarily die. The individual muscular and nervous elements may and do still live for a time—the nerve will conduct a stimulus under which the muscle will contract ; and so it is, even more markedly, with the epithelial cells—those possessing cilia display their characteristic vital actions long after the organism considered as a complex whole has ceased to live.

Now the lower we descend in the scale of living things, the less marked does the life of the organism as a whole become, in contradistinction to the life of its several parts. Schelling's 'tendency to individuation' becomes less and less manifest in proportion as the structural differentiation diminishes. The more the several parts of an organism resemble one another, the less difference is there between the functions discharged by these several parts, and therefore the importance is proportionately less to the whole organism when one of these functions is interfered with. Look at the writhing segment of the worm whose body has been cut by the gardener's spade, or at the green Nereis of the rock-pool whose body has been accidentally torn, and let us think of the powers of repair possessed by each. Look again at the little polyp of our lakes and ponds—the Hydra, whose individual life is so dwarfed in comparison with the life of its several parts that you may cut it or injure it to almost any extent, and yet the separate parts will still live. It can, in fact, scarcely be said to

constitute a living whole, for the one animal may be divided into two, and the two into four or more, and each part will grow into an organism like that of which it is a segment—the parts grow into wholes, and in the place of the one individual organism we get four or more others similar in kind.

These also are the kinds of phenomena and modes of life with which we are familiar throughout the vegetable kingdom—nowhere do we meet with anything like that same amount of integration or individuation which is characteristic of the higher animals. Mere fragments of plants in the form of buds, 'cuttings,' or portions of the root, separated from the parent organism, are as we know capable of reproducing plants similar to those from which they have been derived. The 'tendency to individuation' exists here also, but even in the most perfect plant the accomplished result is small indeed, when compared with what we encounter among animals. The absence of a nervous system, however, combined with the less perfect condition of the vascular system, are sufficient to account for this want of integration in the plant, and the great amount of independence shown by its individual parts.

Such are some of the principal differences in the nature of the 'life,' or aggregate vital manifestations of the members of the animal and of the vegetable kingdoms: and great as are the differences between the phenomena of the higher and of the lower forms of these, we may look for even still lower manifestations of life in a group of organisms whose characteristics, whether structural or functional, are often so little marked as to make the most philosophic naturalists unable to assign them a definite place in either the one or the other of these organic kingdoms.

It might have been expected, in accordance with the doctrines of Evolution, that the lowest living things would present characters of the most general description. They ought to be simply living things, without visible organisation, and should as yet present no special characters by virtue of which a place might be assigned to them either in the vegetable or in the animal kingdom. The older naturalists thought that every living thing must be either an animal or a plant, and they accordingly ranged all organic forms under one or other of these categories. But there were certain of them whose characteristics were so indefinite that they could really claim for themselves, as we have said, no unquestioned

place in either of these kingdoms, and they were consequently placed in the one or in the other alternately as the state of knowledge at the time varied, or almost according to the whim of successive writers. But now, at last, after this unseemly bandying to and fro, their proper position is being generally recognised.

The merit of taking a definite step as regards the classification of these lower organisms rests with Prof. Haeckel, who many years ago said[1]:—"I have made the attempt in my 'General Morphology' to throw some light upon this systematic chaos, by placing, as a special division between true animals and true plants, all those doubtful organisms of the lowest rank which display no decided affinities nearer to the one side than to the other, or which possess animal and vegetable characters united and mixed in such a manner that, since their discovery, an interminable controversy about their position in the animal or in the vegetable kingdom has continued. Manifestly this controversy becomes reduced to the smallest compass if the disputable and doubtful intermediate forms are separated for the present (though only provisionally) both from the true animals and from the true plants, and united in a special organic 'kingdom.' Thereby we obtain the advantage of being able to distinguish both true animals and true plants by a clear and sharp definition, and, on the other hand, a special proportion of attention is attracted to the very low organisms hitherto so much neglected, and yet so extremely important. I have called this boundary kingdom intermediate between the animal and the vegetable kingdoms, and connecting both, the PROTISTA."

It should be understood, however, that in proposing such a classification Prof. Haeckel by no means wished to establish an absolute wall of separation between these three organic kingdoms. He was and is much more disposed to believe that animals as well as plants have gradually arisen out of modifications which have taken place in the simplest Protista. This primordial organic kingdom he divides into ten groups, in the lowest of which, named Monera, are included mere naked, non-nucleated specks of protoplasm.

This discovery and recognition of such simple creatures as the Monera was in many respects a matter of great importance. Formerly the lowest independent living units were thought to be 'unicellular organisms'—that is vesicular units possessing a

[1] 'Monograph of Monera.' Translation in "Quarterly Journal of Microscopical Science," July, 1869, p. 230.

central body known as the 'nucleus' together with a distinct cell wall; and Schleiden and Schwann in 1839, as a result of their remarkable investigations, endeavoured to prove that all the tissues of both plants and animals were entirely built up of such morphological units, named 'cells.'

Some twenty years later Virchow announced[1] views which had an immense influence on pathological doctrines throughout all the schools of medicine, and wherever biological studies were cultivated. He maintained that "the cell is really the ultimate morphological unit in which there is any manifestation of life, and that we must not transfer the seat of real action to any point beyond the cell." He denied altogether the origin of cells *de novo* in blastemata after the fashion described by Schleiden. He held that cells can be produced only from or by pre-existing cells. He did not attempt to prove, however, that the whole bulk of the tissues is made up exclusively of cells; he admitted the existence of a large amount of intercellular material in many tissues, and so, in order to reconcile this fact with his previous doctrine, he was compelled to put forward the hypothesis that such intercellular material may be broken up into imaginary 'cell territories,' each of which is influenced or ruled over by the cell lying in its midst.

But important modifications as to the true conception of the nature of a cell had been growing up even before the publication of Virchow's theories. So far the cell has been spoken of as an altogether definite structure: as a body with a distinct wall or bounding membrane and certain contents, including among other things one of the most fundamental parts of the cell, the 'nucleus.' But it was maintained by Nägeli,[2] and also by Alexander Braun[3] and then more emphatically by Max Schultze, that a distinct investing membrane or cell-wall was not an essential character.[4] Afterwards the typical ultimate unit of life was still further shorn of its characteristics when it was shown (if this had not been already done by Nägeli and Braun) by Brücke,[5] Kühne,[6]

[1] 'Cellular Pathologie,' 1858.

[2] 'Zeitschrift für Wissen. Botanik,' 1846 (Transln. Ray Soc. 1849, p. 95).

[3] 'The Phenomena of Rejuvenescence in Nature,' 1851 (Transln. Ray Soc. 1853).

[4] 'Reichert and Du Bois Reymond's Archiv,' 1861. A mere mass of protoplasm with a nucleus was sufficient to constitute a 'cell'; and at the same time it was maintained that the substance of the cell (or that within the wall, where this existed) was *protoplasm*, a contractile substance answering to what Dujardin had named *sarcode*.

[5] 'Wiener Sitzungsberichte,' 1861, pp. 18–22.

[6] 'Protoplasma und die Contractilität.' Leipzig, 1864.

and others, that even the nucleus was a non-essential constituent of such a body.

So that in place of the old 'cell' with its definite characters, this would reduce us to a mere naked, non-nucleated bit of protoplasm as the simplest material substratum adequate to display all those 'vital' manifestations which were previously considered as the essential attributes of certain formed elements known as 'cells.' The power of displaying vital manifestations was, in fact, in opposition to the views of Virchow, transferred from definitely formed morphological units to indefinite and formless masses of what is called protoplasm. Instead, therefore, of an obvious form of 'life' we were reduced to a matter of 'life' presenting no appreciable morphological characters. For such a structureless unit of protoplasm—a mere shred of plasma or living matter—Haeckel proposed the term 'cytode'; reserving the term 'cell' for such a unit when it contains a nucleus—though it must be understood that it, as well as the cytode, may be either naked or bounded by some sort of limiting membrane.[1]

The old doctrine as to the fundamental properties of the 'cell' as the vital unit, did well enough in those days when the lowest known living things—the lowest plants and the lowest animals—were thought to be 'unicellular organisms.' But since our knowledge has increased, since we have become more familiar with the various living things now constituting the lowest groups of the third organic kingdom—the Protista—the maintenance of such doctrines (leaving aside all other reasons) had become impossible. We have seen that although there are multitudes of amœboid organisms, possessing the old cell characters—that is, having a distinct nucleus and a more or less definite limiting membrane—there are, nevertheless, adult animals entering into the composition of the lowest genus of this kingdom—the *Monera*—which may have no bounding membrane and no nucleus, being mere bits of protoplasm, naked, non-nucleated and structureless. While there are others, such as the Bacteria and the blue-green Chromacea, which though equally devoid of a nucleus are possessed of a more or less definite limiting membrane. Yet, such minute, homogeneous units of protoplasm are as capable of displaying the fundamental characteristics of 'life' as are the more definite unicellular organisms, to which such attributes were previously supposed

[1] The term 'plastide' is used by some rather than 'cytode,' though the latter nomenclature is more commonly adopted.

to be restricted. Without visible structure they nevertheless assimilate materials from their environment, and grow; some of them constantly vary in form, and are capable of executing more or less rapid movements; though possessing no nucleus they, nevertheless, are able to divide and to reproduce their kind—these being, as we have seen, the fundamental properties of living matter.

Thus the cytode is the elementary vital unit and the cell is a product of a higher order which may be, and constantly is, developed therefrom. The first stage towards the development of a typical cell occurs when the plastide undergoes some condensation of its outer layer so as to form a limiting membrane; while the second and essential stage is shown by a differentiation of the contents of the primitive vital unit, and the gradual appearance therein of the structure known as the 'nucleus,' to which an enormous importance is now attached, perhaps in some respects in excess of that which it deserves.

However Weismann and his school would interpret the fact, or reconcile it with their views, there is really no room for doubt that in multitudes of plastides of various kinds a nucleus appears where previously there was none.[1] It usually reveals itself, moreover, in the manner originally described by Alex. Braun in his celebrated "Rejuvenescence in Nature."[2] During the development of reproductive units in Hydrodictyon and other algæ he observed it appearing as a mere minute 'clear space'—that is a microscopical speck of matter free from granules—in the midst of a protoplasmic unit. This mode of development of a nucleus the writer has himself often seen in plastides in which previously there was no trace of such a body. Thousands of such plastides may often be seen developing in the pellicle on a hay infusion, as will be described in a subsequent chapter (see p. 196 and Figs. 24, 25). Later, the nucleus also acquires a limiting membrane, and undergoes other important developmental changes to which we shall subsequently have occasion to refer. The 'cell' is thus seen to be only a developed form, a more visibly complex condition which a simpler but already living and independent unit may assume.

But even before the discovery of the simple Monera by Haeckel it had been long previously recognised by Nägeli, and also by

[1] Weismann goes so far as to say that a nucleus never arises *de novo* ("The Evolution Theory," Transln. 1904, Vol. I, p. 309).

[2] Transln. by Henfrey (Ray Society), 1853, p. 261.

Braun, that nothing like a nucleus was to be found in many Algæ. Thus the latter says (*loc. cit.* p. 174) "in particular families of the Algæ, as, for example, in the Palmellaceæ, Chlorococcaceæ, Oscillatorieæ, and Nostochineæ, as also in the large-celled Cladophoræ, and the unicellular Algæ with unlimited growth of the cell (Vaucheria, Codium, Caulerpa), no trace of a nucleus has yet been discovered." Yet in these latter so-called (but wrongly so-called) 'unicellular' Algæ, and in the others just mentioned, all the fundamental properties of living things—even in some of them (as in Vaucheria) the formation of active ciliated reproductive units —are manifested, notwithstanding the absence of a nucleus. It is useless, therefore, as was formerly the case and as happens only too frequently now, to attribute all the important phenomena occurring within a cell to the effects of its influence. Still, though it is clear that no such structure is necessary to enable a unit of protoplasm to exist as an independent vital unit, it has been found, as we shall see later on, that where present it exercises a most important influence in the production of one of the fundamental phenomena of living matter—namely, the act of assimilation, whereby its own proper structure is built up by a living unit during its process of 'growth.'

We are now in a position to speak briefly concerning the various fundamental properties of elementary living units; and what we shall have to say will not concern cytodes only, but will be applicable also to 'unicellular organisms,' and will go to show how closely, in each case, their vital manifestations are related to, and correlated with, ordinary physico-chemical processes.

This subject has been very ably and logically treated in a little book by F. le Dantec, entitled "La Matière Vivante,"[1] to which several references will be made.

(1) One of the first and most striking characteristics of the lowest forms of life, as viewed under the microscope, is the power possessed by many of them—though others do not possess it—of what is *apparently spontaneous or instinctive Movement.* And it is precisely this appearance of spontaneous movement which has tended to foster the notion that even in such elementary units their movements are due to an "internal vital principle." But, as a matter of fact, these movements, like other elementary manifesta-

[1] Paris, 1895. One of the Vols. of the "Encyclopédie Scientifique des Aide-Mémoire."

tions of life, result from the physical and chemical reactions taking place between the substance of which the living units are composed and the medium in which they exist.

It is well known in regard to their medium that more or less oxygen always exists in a state of solution in such a fluid, and that the substance of the living units is constantly undergoing oxygenation and giving off carbonic dioxide, in addition to or as an integral part of, the other concomitant chemical processes concerned in the assimilation of materials from the medium—whereby its own substance is built up and growth occurs. Verworn has shown that if free oxygen is withdrawn from an infusion the movements of Amœbæ and other low organisms gradually cease; but that, even after many hours, the re-addition of oxygen causes their several movements to recommence. Oxygen acts like this upon all sorts of cytodes and unicellular organisms and as le Dantec says (*loc. cit.* p. 59), "it is very probable that the majority of the normal movements of plastides are due to the constantly changing distribution of oxygen in the infusions."

It is owing to the fact that light influences these chemical reactions that this agent seems to exert a direct effect upon the movements of many of the lower forms of life, causing them, as the anthropomorphists say, to 'seek' the light. This phenomenon is seen especially with many Bacteria, with spores of Algæ, Diatoms, Desmids, Euglenæ and other allied organisms, together with flagellate Monads. The movements in the great majority of cases are towards the source of light—rarely away therefrom, but always in relation with the direction of the luminous radiations. The actual explanation of the movement seems to depend upon a relative exaltation in the activity of the chemical processes on the side of the organism on which the light impinges, as le Dantec shows; the general effect being, as it were, to draw the organisms towards the source of light.

Then again food-stuffs, as well as chemical compounds of various kinds, are now known to exercise a remarkable attractive influence upon cytodes and unicellular organisms—varying much in different cases both from the point of view of the nature of the organism and the nature of the compound—but in all cases doubtless dependent upon the particular chemical activities capable of being excited in the several kinds of living units. These differences, as well as the different kinds of movement, or the absence of movement, and the different behaviour towards light being, as

le Dantec very properly insists, so many evidences pointing to the conclusion that the varieties of protoplasm in these elementary organisms are almost endless.[1]

The capability shown by unicellular organisms of being attracted by food-stuffs was first carefully studied by Stahl in 1884, but according to A. Fischer,[2] "At about the same time Pfeffer investigated from a more general chemical standpoint the effects of such stimuli upon the movements of bacteria, protozoa, and the spermatozoids of the higher cryptogams, and was able to show that the nutritive value was not the sole and only determinant, but that other more recondite factors were involved, factors based on the chemical constitution of the substance employed as a stimulant. For this reason he introduced the now generally adopted expression Chemotaxis."

Pfeffer's experiments were conducted in a very exact manner. He introduced solutions of different materials, varying in their degrees of concentration, into very short capillary tubes closed at one extremity. A tube thus prepared was then immersed in a drop of the fluid containing the plastides whose behaviour was to be examined. Owing to the capillary bore of the tube diffusion of the fluid only takes place at its orifice, and then slowly. Let us suppose it to have been charged with a solution of peptone, and then immersed in fluid containing Bacteria, which it should be understood are simple cytodes that move by means of cilia as do the swarm-spores of Vaucheria already referred to. The results of this and other experiments are thus referred to by Fischer :—"In five or ten seconds it can be seen that the bacteria are more thickly congregated around the open end of the capillary than elsewhere, and in a few minutes they form a dense swarm there, and begin to enter the tube. The movements of the bacteria in the drop become more lively as soon as the diffusing peptone reaches them, and at the entrance of the tube they are a whirling, 'buzzing' mass, like hiving bees. The potential energy of the food-stuff has been converted into the kinetic energy of the vibrating cilia. If now a cover glass be laid upon the drop another kind of Chemotaxis can be observed. The bacteria which have entered the tube move upward, attracted by the air in the blind end, and in about half an hour that section of the capillary immediately below the bubble is plugged by a thick mass of organisms. But these phenomena, the

[1] *Loc. cit.* pp. 31, 43, 54, 61.

[2] "The Structure and Functions of Bacteria," Transln., 1900, p. 78.

attraction by food and the attraction by air might as well be called trophotropism as chemotaxis, the latter being shown in its purest form when solutions of salt are employed. For instance, a 1·9 per cent. solution of KCl has a powerful effect, and draws the bacteria into the tube just as peptone does, being still feebly attractive even in a dilution of 0·019 per cent. Among the alkalies, potassium is chemotactically the most powerful, then sodium and rubidium. The alkaline earths are less effective. . . . Among organic substances with a high nutritive value, asparagin and peptone may be mentioned as being strongly chemotactic, whilst sugar, one of the best food-stuffs and richest sources of energy has but little attractive power. Glycerine is in all cases, as far as is known, inactive. . . . Contrasting with the phenomena just described is the power which certain substances have of *repelling* bacteria. This is known as *negative* chemotaxis. Free acids and alkalies have this effect, and capillaries filled with their solutions invariably remain empty. . . . In the case of negatively chemotactic compounds, the poisonousness of the substance is no more a criterion of its repellent power than the food value is an index of the attractive power in positively chemotactic bodies. For instance, a solution of 0·019 per cent. potassium chloride plus 0·01 per cent. mercuric chloride attracts bacteria by reason of the potash it contains, but they rush into the tube only to meet their death from the mercury salt."

(2) In many cases undoubtedly these chemotactic processes are intimately associated with the nutrition of the cytodes and unicellular organisms whose movements are thus incited, and to the processes included under this head we now turn. They are *Assimilation* alone, and *Digestion followed by Assimilation*—understanding 'assimilation' to be the process by means of which matter from without, of some kind, is converted within the living unit into the special kind of protoplasm of which such unit is composed.

It was, as Fischer points out, formerly an axiom of general physiology, "that of all organisms the green chlorophyll-bearing plants alone are able with the aid of sunlight, to assimilate the carbon dioxide of the atmosphere and from it to build up carbohydrates. All other living things, that is to say, plants devoid of chlorophyll, bacteria, and all forms of animal life, were supposed to be dependent for their carbon, indirectly or directly, upon the carbon compounds already formed by green plants." But then he

goes on to say (*loc. cit.* p. 47) :—"The integrity of this principle has now been destroyed by the discovery of certain bacteria in the soil, which are able, even without sunlight, to appropriate the carbon dioxide of the atmosphere." These are the nitrifying bacteria, present in every variety of soil, which were first isolated in 1889 by a Russian investigator Winogradsky, and are "characterised by an extremely primitive metabolism, a physiological humility which shows them to occupy the very lowest rung of the ladder of life."

But the fact recorded by the writer in 1871 that Bacteria were capable of multiplying freely in a simple solution of ammonic tartrate in distilled water as mentioned in the last chapter (p. 18) showed that other common Bacteria and some Torulæ possessed closely allied properties, of an even simpler order. Speaking of the nitrifying Bacteria Fischer says, "The materials from which they build up their cells are therefore inorganic compounds of the very simplest character, carbon dioxide and ammonia or nitrous acid with a few mineral salts. They are thus prototrophic in the strictest sense of the word, for a simpler synthesis of proteids than theirs is scarcely conceivable." It cannot be said, however, in this case that sulphur and phosphorus were absent, as they may have been ingredients of the "few mineral salts," but in the case made known by the writer so much earlier (in 1871) the Bacteria and the Torulæ have in some way to build up their substance from the elements entering into the composition of distilled water, tartaric acid and ammonia—and *this* surely must be the simplest known mode of origin of protoplasm, as C, H, O, N seem alone to have entered into its composition. Yet any one can test its reality by adding a single drop of a turbid hay or beef infusion to an ounce of distilled water in which ten grains of neutral ammonium tartrate is dissolved. If this solution thus inoculated with common micro-organisms is kept at a temperature of about 80° F (27° C.) for a couple of days the clear fluid will have gradually become opalescent from the growth of myriads of Bacteria, and perhaps some Torulæ, and after a time it will become quite turbid as their numbers increase.[1]

[1] In an experiment of this kind, recently made, in which a drop of a hay infusion was used as the inoculating agent, the tartrate of ammonia solution only became very slightly opalescent even after several days, but on examination this opalescence was found to be due almost solely to minute single and budding Torulæ. See Note 1, p. 69; and 'Knowledge,' Aug., 1905, p. 199, for some additional details in regard to this whole subject.

The assimilative processes of Chromacea and many unicellular Algæ and Phytozoa are only a little more complex, since they build up their substance in the main after the fashion of higher plants, owing to their power of causing mere inorganic compounds to fall into those modes of combination that exist in their own particular kinds of protoplasm. In other of these latter organisms, however, as with Euglenæ and some of their allies, organic compounds, existing in their media in a state of solution, are in part made use of in their assimilative processes.

We have, in these cases, a transition to the assimilative processes carried on by the great majority of Bacteria which are commonly supposed to be wholly unable to build up their protoplasm from inorganic substances, and are dependent upon organic compounds existing in the tissues or secretions of plants and animals—either during their life or after their death—or upon products which have diffused therefrom into the outside world. Much the same kind of processes must also obtain with the multitudes of Protozoa that exist as parasites within the various organs and tissues of almost all kinds of animals—where they find themselves surrounded by assimilable organic compounds which may pass into their substance by osmotic processes, but still have to be coerced in each case into the particular combinations existing in the special variety of protoplasm entering into the composition of the parasite. In this respect, however, they are only exercising the same kind of power as that which is ever being exerted in every tissue of our own bodies. The blood is the common pabulum carried to all, and from it the different kinds of epithelial cells, of gland cells, of nerve cells, and others abstract the particular materials needful to build up their own special kinds of protoplasm. It is usually assumed that such processes are simpler and easier than those effected by the prototrophic Bacteria, as they are capable of using the simplest inorganic materials out of which to fashion their own peculiar life-stuff. As to the relative simplicity of the two processes, however, we need not venture to offer an opinion ; though it seems clear that in each case we have to do with mere chemical combinations brought about by contact action—'catalysis' as it is termed—concerning which a few words must now be said.

The remarkable discovery was made by Berzelius in 1810 that certain bodies, by their mere presence, were capable of bringing about particular chemical combinations in substances or elements brought into relation with them, without being themselves affected.

Thus under certain conditions dilute sulphuric acid will change starch into dextrine and sugar without undergoing any alteration itself; while finely divided platinum brought into contact with oxygen and hydrogen will cause them to combine and form water.

Later, it was ascertained that catalysis, or "contact action" as it was termed by Mitscherlich, is also a property possessed by the great class of soluble ferments known as 'enzymes,' which play such an important part in fermentations and in very many of the life-processes of organisms—these enzymes being complex chemical products elaborated during the life of cells, in their own substance, where they are sometimes retained and at others excreted. The investigations of Verworn, Hoffmeister, Ostwald, and others have shown what an important part the catalytic actions of enzymes play in the metabolic processes carried on in the body by cells of different kinds. Ostwald for instance in a discourse "On Catalysis" delivered in 1901 at Hamburg said:—"We must recognise the enzymes as catalysators that arise in the organism during the life of the cells, and by their action relieve the living being of the greater part of its duties. Not only are *digestion and assimilation* controlled by enzymes from first to last, but the fundamental vital action of most organisms, the production of the necessary chemical energy by combustion at the expense of the oxygen in the air takes place with the explicit co-operation of enzymes, and would be impossible without them." Verworn also advocates the importance of catalysis in the work of assimilation, as carried on by certain ultimate minute units of living matter which he terms 'biogens,' and agrees with Ostwald in regarding the processes that occur as essentially physico-chemical in nature.[1]

So far we have been dealing with cases in which the simplest living units avail themselves of materials—either inorganic or organic—in a state of solution; but in multitudes of other cases solid food is taken which requires to be dissolved ('digested') before it is assimilated. This happens for instance with all kinds of Amœbæ, with Ciliated Infusoria, with Sun Animalcules and others. Where the food masses so taken are small they are commonly received into small cavities or 'vacuoles' in the body substance of such organisms, and within such vacuoles they gradually become dissolved, the products of solution ultimately

[1] See his "General Physiology," 1899, Transln., pp. 481–6, and 529. This subject is also fully discussed in the work of Reynolds Green on "Soluble Ferments," 1899, Chap. XXIV.

passing by dialysis into the surrounding protoplasm, by which they are assimilated. All the steps of this process, including an account of the movements of Amœbæ, the mode in which they take foreign bodies into their interior, the origin of the vacuoles, and what goes on therein, are well told by le Dantec (*loc. cit.* pp. 65–104), but need not detain us here.

So far as the process of 'digestion' is concerned it is admitted by all that the phenomena are essentially chemical in all cases, and that with the aid of enzymes the process can be carried on outside the body, under suitable artificial conditions, with perfect ease. There is no mystery here—the mystery, in the sense of our ignorance of the exact steps of the process, rests rather with the subsequent assimilative processes which, as we have indicated, are similar in kind whether we have to do with independent living units or with the constituent elements of higher organisms.

In certain cases, however, both Amœbæ, Ciliated Infusoria, and Sun Animalcules take into their bodies comparatively large masses of food, which may not be contained in vacuoles, but may be in immediate contact with their own protoplasm, where it becomes rapidly transformed and assimilated. Nothing is more astonishing than the capacity of this kind displayed by many Amœbæ or Ciliated Infusoria. The writer has several times seen a large Amœba containing in its interior a Rotifer of half its own bulk undergoing digestion; while it is the commonest thing possible to see in decaying cells of Nitella certain Amœbæ growing rapidly as they gorge themselves with Chlorophyll corpuscles, till at last they form motionless spheres so densely packed with chlorophyll that nothing else can be seen except a limiting membrane, as shown in the writer's "Studies in Heterogenesis" Pl. XVI., fig. 164; and yet, as a reference to p. 251 of that work will show, in the space of no more than ten hours the whole of that chlorophyll, with progressive changes in colour, becomes rapidly digested and assimilated—converted, in fact, into colourless protoplasm, which within the same space of time has, moreover, often more or less completely segmented into Monads.

The rapidity of this process is so surprising as to be almost incredible to one who has not himself seen it occurring. Yet almost exactly the same kind of thing occurs with specimens of a Ciliate belonging to the genus Otostoma, found at times in decaying Nitella. These creatures also gorge themselves with chlorophyll till they appear only like great, green ciliated balls;

then they come to rest, encysting themselves, and begin rapidly to digest the comparatively enormous mass of food which they have taken—probably by means of some very powerful enzymes formed in their interior. Processes of discolourisation go on as in the Amœba, the food is dissolved, and apparently assimilated as it is dissolved, so that after the expiration of about two days we may see within the cyst perhaps a little pigmentary refuse material which has been cast out, but otherwise only colourless protoplasm in the form of two or four Ciliates, into which the previous single individual has divided. The result is truly amazing, alike from the amount of matter that is disposed of and the rapidity of the metabolic processes by the aid of which the vegetable protoplasm becomes animalised.

(3) We now have to turn our attention to the last of the fundamental characteristics of living matter, namely its power of undergoing processes of *fission*, such as those to which reference has just been made, whereby 'reproduction' in its simplest form is brought about. This mode of reproduction differs little from growth itself; it was, in fact, long ago aptly termed by Huxley "discontinuous growth."

In regard to the problem as to the cause of this discontinuity, or what brings about the spontaneous division which is so prone to occur in living units of various kinds, the reader will be in a better position to form some idea after we have briefly dealt in a later chapter with the question of the "Molecular constitution of living matter." It must suffice to say here that it is commonly recognised both by chemists and biologists that protoplasm must be made up of molecules of the most extreme complexity, the constituents of which are in a constant state of flux and change, often merely from one isomeric state to another. A condition of unstable equilibrium, in fact, is presumed to exist, constantly varying in response to changes in the medium. And it is assumed that for each of the innumerable different varieties of what we call protoplasm there are, when in this or that medium, certain varying but not very precise limits of size, beyond which the molecular activities of the plastide or cell seem no longer consistent with the existence of only one centre. Two centres, therefore, in some way become established, and the attractive influences associated therewith cause the semi-fluid substance of the protoplasm to gather round each of these centres till 'discontinuity' is established. The

halves thus produced then increase in size (or 'grow') by means of their own proper assimilative processes, till they, in their turn, attain such a size as (having regard to the qualities of the medium at the time) seems again to necessitate in each the formation of two centres around which the plasm gathers till discontinuity is once more brought about, and so on almost *ad infinitum*.[1]

Whatever may be the exact chemico-physical processes concerned in this act of fission or segmentation the important thing to be borne in mind at present is that it is a property which belongs to living matter itself, and that it is not dependent upon any visible form or structure. This is obvious from the fact that it is manifested by Bacteria, by amœboid Monera, and by multitudes of Algæ belonging to the family of Chromacea, as well as by other non-nucleated plastides. Further it is now known to occur even in such units as chlorophyll corpuscles; while my researches have shown that it is again an ever recurring process in the aggregates of Bacteria, constituting 'Zooglœa' masses, which form so abundantly on the surface of a hay infusion.[2]

It is perfectly clear, therefore, that nuclei have no necessary relation with this process of fission, and that it is to be regarded as one of the fundamental properties of living matter itself—even where this is devoid of all visible structure or organisation.

The question is altogether a different one as to what part the nucleus plays in the process of fission as it occurs in those living units in which such a body is present. Is it in any way causative then, or is its own fission merely a concomitant result of the sum total of molecular activities occurring within the cell, when it is approaching its natural limits of size?

Disregarding, or not adequately considering, the fact that in such multitudes of cases the fission of plastides occurs where a nucleus is absent, it is common to ascribe to the nucleus a causative influence when fission occurs in a cell. And this view is undoubtedly fostered by the fact that division of the nucleus commonly commences before, and proceeds more rapidly than, the division of the substance of the cell itself. This holds good

[1] On the other hand, in many filamentous Algæ and Fungi, instead of complete fission the new cells formed may remain in contact separated from one another by dissepiments. A similar incomplete segmentation is met with among mere plastides, the most familiar examples of which are elongated Bacilli or Streptococci.

[2] See "Studies in Heterogenesis," 1903, pp. 65–75.

for the simple amiototic division, as well as for the vastly more complicated process of karyokinesis to which no further reference will be made at present.

Although we know that fission need not depend upon the presence and influence of a nucleus, it does not necessarily follow that when such a body has been evolved, it may not be in the unit that possesses it in some way causative of such a phenomenon as fission. Too much doubt and uncertainty exists at present as to the real functions of the nucleus to enable us to come to a well-founded opinion on such a point. The fact that division of the nucleus precedes division of the cytoplasm is by no means conclusive, since there is good reason for believing that its molecular composition is more complex and unstable than that of the cytoplasm, and this might be the reason why, when the sum total of molecular changes in the cell are about to eventuate in the formation of two separate centres, such processes should take effect sooner, and proceed more rapidly, in the nucleus than in the body of the cell itself.

The nucleus is a developmental product resulting from differentiation of the cell contents, and it is only natural to suppose from its comparative universality and from the complicated processes taking place in connection with it, when what is known as karyokinesis occurs, that it must be associated with important functions. The results of numerous experimental researches carried on with unicellular organisms of various kinds by Balbiani, Verworn and others, in which sections of such organisms have been made have led to important conclusions. 'Merotomy' is the term applied to these researches, and they have invariably shown that segments which do not contain a nucleus do not survive; while those that contain the nucleus live and speedily regenerate their primitive form. This has been shown to hold good for vegetal cells, for naked Rhizopods, as well as for Foraminifera and for Ciliated Infusoria,[1] so the conclusion seems inevitable that where we have to do with nucleated cells the process of 'assimilation' is only carried on in those segments that chance to contain a nucleus. Growth seems impossible where the nucleus is absent —though only where it is altogether absent, as the remarkable experiments of Balbiani with specimens of Stentor have shown.

[1] Balbiani discovered one remarkable exception among these latter organisms, since the nucleated merozoïtes of Paramecia, though they lived for a month or more, showed no evidence of regeneration.

This rather large trumpet-shaped Ciliate possesses a long, moniliform nucleus, and Balbiani [1] carefully studied the question as to the amount of this nucleus existing in a fragment of the organism which would suffice, in order that regeneration of the form and structure of the organism from such a fragment might occur. The account given of these experiments is as follows :—

"A Stentor having been divided transversely into three segments, an anterior one containing a nuclear chaplet of six grains, one containing only a single grain, and a posterior segment having four grains, by the next day all three were completely regenerated into perfect Stentors; and though the middle fragment, with its single nuclear grain, had to undergo much more numerous and more profound transformations than the other two, its regeneration had taken place in the same time as that of the others, and it had become just as perfect a Stentor—only smaller, on account of the difference in size of the three segments at the time of section. . . . In other analogous experiments the single nuclear grain was contained in the posterior, or in a longitudinal, fragment of the body, and the results have always been similar. We must conclude from these facts that the particular quantity of nuclear substance existing is of no importance, either for the degree of perfection or for the progress of the regeneration, and that a single grain of the nucleus behaves in this respect just like an entire nucleus."

We are warranted in concluding from these experiments, as le Dantec does, that the nucleus seems to act like a chemical substance rather than as an organ, since, as he says, "a *portion* of the nucleus in a *portion* of the corresponding protoplasm brings about the same syntheses as an entire nucleus in an entire plastide." This is an important conclusion from the point of view of the functions of the nucleus, and is fairly compatible with the suggestion made by Herbert Spencer [2] that having regard to its chemical complexity and relative instability "the nucleus is a source of perpetual molecular disturbance—not a regulating centre but a stimulating centre." Nothing further on this subject need, however, be said for the present. We may now turn from this digression for a few concluding words upon the subject with which we are more immediately concerned, namely, the property of spontaneous fission, as a fundamental characteristic of living matter.

[1] "Ann. de micrographie," 1892 and 1893.

[2] "Principles of Biology," Revisd. Edn., 1898, Vol. I, p. 260.

It may be asked, in conclusion, whether this is a property *sui generis*, or whether it may be manifested by any kinds of not-living matter? And here again it must be said that even what appears to be such a peculiarly vital phenomenon as spontaneous fission may be manifested by some kinds of not-living matter.

It has been long known that 'myeline' a very complex fatty compound obtainable from yolk of egg and from nerve substance will, when immersed in water, as Montgomery[1] has shown, give rise to cell-like bodies and also protrude delicate tubes which bend in all directions. But Robin[2] long ago showed more remarkable processes in the way of actual fission and amœboid changes in shape occurring in other fatty extracts, derived from dead animal substances. These phenomena were especially marked with certain fatty extracts obtained from the blood, when mixed with water or with albuminous fluids. "From masses of these extracts," Robin says, "there may be seen projecting and elongating under the eyes of the observer filaments having a tubular appearance, either straight, bent, undulating or spiral in their arrangement, like those of various anatomical elements. Sometimes the extremities of some of these tubes become constricted and moniliform, and the constrictions go on so as to produce complete division, with separation of little hollow spheres, just as in the production of conidia from the tubular cells of various Moulds, Oidium, etc. . . . When little spheres, or drops with a wavy outline, are formed, these may be seen under the microscope, not to bend now in this now in that direction like the tubular filaments just referred to, but incessantly to change their form, as a result of alternate partial constrictions and dilatations. These constrictions or contractions even go so far as to produce a complete division of certain globules into two, in the same way that one may see division brought about by gradual constriction in certain vegetal or animal cells." Such fatty extracts are compounds having a very high molecular complexity, so that these spontaneous divisions and changes in form can only be ascribed to internal molecular movements of a much simpler kind, doubtless, but in some measure akin to those occurring in living matter when fission occurs or amœboid movements are being produced.

[1] "On the Artificial Formation of so-called Cells," 1867.

[2] "Mem. de l'Acad. de Médicine," 1859, p. 248; quoted also in his "Traité du Microscope," 1871, p. 562.

CHAPTER III

ON SOME PROPERTIES OF CRYSTALS, WITH OBSERVATIONS ON THEIR MODE OF ORIGIN AND ON THE MODE OF APPEARANCE OF LIVING UNITS

JUST as the form and properties of the crystal are to be taken as the natural outcome of the properties of its constituent molecules under the influence of its environment, so are the forms and properties of simplest organisms to be considered as the natural outcome of the properties of their molecules, entering into combination under the influence of their environing conditions.

Views analogous to these have been more or less fully expressed by many writers. The essential similarity in the laws regulating crystalline and organic forms was even suggested by Maupertuis in 1744.[1] Crystals and organisms were spoken of by Burdach[2] as statical and dynamical aggregates respectively. The formation of organisms was, moreover, in 1836, definitely compared by Schwann to the formation of crystals. Cells, which were then believed to be the types of all primordial organisms, were thought by him to owe their form to a process essentially similar to crystallisation; the characteristic shapes being due, in the case of cells, to a peculiarity in the nature of the substance of which they were composed. But it was in 1863, in the first edition of his "Principles of Biology," that this relationship was most fully pointed out by Herbert Spencer, believing as he did that the structures and shapes of lower organisms are the results of the 'polarities' of their constituent organic units, under the continually modifying influence of external conditions. To his views on this subject we shall subsequently refer in some detail, so nothing more need be said at present.

Although saline materials so frequently aggregate into crystalline shapes when they emerge from the state of solution, still, in many

[1] See Milne-Edwards in "Physiologie et Anatom. Comp." t. viii. p. 247.

[2] "Traité de Physiologie" Transln., 1839, t. iv, p. 129.

cases, the assumption or not of such a form is entirely dependent upon the conditions under which the separation takes place.

Many substances which, in the chemist's laboratory, are only seen in the form of insoluble precipitates made up of amorphous granules, could have been procured in a crystalline condition, if the same decomposition which had given rise to the amorphous precipitate had been allowed to take place more slowly. If instead of pouring a certain amount of a solution of sulphate of potassium into one of chloride of barium, we allow the mixture to take place gradually by means of dialysis, then crystals of sulphate of barium are formed rather than an amorphous precipitate. It has, in fact, been ascertained by Frémy[1] that insoluble compounds generally, which appear in the laboratory as a result of double decomposition in the form of amorphous precipitates, can almost invariably be obtained in a crystalline condition when the chemical reaction is allowed to take place very slowly. This may be brought about by making the saline solutions mix after osmosis through membranes, wooden vessels, or porous porcelain. By one or other of these methods, he obtained many very insoluble salts in the crystalline condition—such as the sulphates of baryta, strontia, and lead, the carbonates of baryta and lead, oxalate of lime, chromate of baryta, and several sulphides.

Variation in the 'conditions' under which the crystallisation of any particular substance occurs, moreover, often gives rise to the most marked variation in its crystalline form. Thus, referring to the article 'Dimorphism' in Watts's "Dictionary of Chemistry," we find the following statements:—"Many substances, both simple and compound, crystallise in forms which belong to two or three different systems of crystallisation, or which, even if they belong to the same system, yet exhibit such differences in their corresponding angles as to render it quite impossible to reduce them to the same form: this was first shown by Mitscherlich, in 1823 (Ann. Ch. Phys. [2] xxiv. 264). Such bodies are said to be dimorphous and trimorphous. The difference of crystalline form which they exhibit is associated with difference of specific gravity, hardness, colour, and other properties. Whether a body shall crystallise in one system or another seems to depend chiefly upon temperature. . . . Sometimes the form of the crystal varies according to the solvent from which it separates: thus arsenious anhydride

[1] "Compt. Rend." t. lxiii. p. 714.

crystallises from water or hydrochloric acid in regular octahedrons, but from alkaline solutions in trimetric prisms." Taking some other specific instances, we find that—" If a solution of carbonate of calcium in water containing carbonic acid be left to evaporate at the ordinary temperature, nothing is obtained but calcspar, in microscopical, and, for the most part, truncated primitive rhombohedrons; if, on the contrary, the solution be evaporated over the water-bath, arragonite is obtained in small six-sided prisms—mixed with a few crystals of calc-spar, because the temperature of the solution is lower at first than it afterwards becomes."

In other examples we may find not only a striking difference in physical form, but also a notable change in colour, occasioned by the molecular rearrangements which the change of temperature seems to necessitate. Thus I have before me now a sheet of paper having a large circular area smeared over with a scarlet patch composed of the double iodides of mercury and copper ; if I hold it over a lamp, almost immediately the scarlet patch assumes a brownish-black colour ; if the exposure has been only momentary, the patch almost immediately, on withdrawal, again assumes the scarlet colour ; if I allow it to remain exposed to the heat for five seconds it takes about fifteen seconds for the brownish-black colour to disappear and for the scarlet colour to be restored. These changes will recur over and over again invariably, and just as they did more than thirty years ago when the specimen first came into my possession.

It has also been found that the whole nature of a crystal already in existence may be changed by the action of causes which seem the most trivial : a slight elevation of temperature, or even the most delicate touch, in some cases, is capable of initiating changes which spread through their entire substance, or throughout a whole aggregate of cohering crystals. Thus in the same article on 'Dimorphism' to which we have already referred, we find the following statement :—" Crystals formed at one particular temperature, and then exposed to that temperature at which crystals of a different kind are produced, often lose their transparency, and *without alteration of external form, become changed into an aggregate of small crystals of the latter kind :* examples of this alteration of structure are afforded by sulphur, carbonate of calcium, mercuric iodide, and many other bodies." Again :—"*Mercuric iodide* separates from solution, and likewise sublimes at a very gentle heat, in scarlet tables belonging to the dimetric system ; but when sublimed

at a higher temperature, in sulphur-yellow, rhombic tables of the monclinic system. The red crystals turn yellow when heated, and resume their red tint on cooling. The yellow crystals obtained by sublimation retain their colour when cooled ; but, on the slightest rubbing or stirring with a pointed instrument, the part which is touched turns scarlet, and *this change of colour extends with a slight motion, as if the mass were alive, throughout the whole group of crystals as far as they adhere together*." Then again :—"*Nitrate of potassium* usually crystallises in the form of arragonite : but if a drop of the aqueous solution of this salt be left to evaporate on a glass plate and the crystallisation observed under the microscope, it will be found that, side by side with the prismatic crystals at the edge of the drop, a number of obtuse rhombohedrons of the calcspar form are produced, just like those in which nitrate of sodium crystallises. As the two kinds of crystals increase in size and approach one another, the rhombohedrons become rounded off and dissolve, because they are more easily soluble than the others, while the arragonite-shaped prisms go on increasing in size. When the two kinds of crystal come into immediate contact, the rhombohedral ones instantly become turbid, acquire an uneven surface, and after a short time throw out prisms from all parts of their surfaces. Contact with foreign bodies also brings about the transformation of the rhombohedrons while they are wet. If the drops are so shallow that the liquid dries round the rhombohedrons before they are disturbed, they will remain for weeks without disintegrating, and bear gentle pressure with foreign bodies without alteration ; but stronger pressure, or scratching, or the mere contact of a prismatic crystal of saltpetre, causes them to change."

These facts, together with those already cited, seem to show clearly enough, not only that the crystalline form of any crystallisable material is variable to a remarkable extent when it is allowed to crystallise under different conditions ; but that, even when formed, a crystal produced under a certain set of conditions may be compelled by its very nature, when these are changed, to undergo an entire molecular rearrangement before a polar equilibrium can be again established between the same molecules and the new influences to which they are subjected. It will be well that all such facts should be thoroughly borne in mind when we have to consider the transformations that occur, or that may be deemed possible, in simple or elementary forms of living units: for

if the combinations constituting 'living matter' are very much more complicated and unstable than those of the crystals to which we have just been referring, then the forms assumed by such sensitive matter, under different conditions, ought to be excessively variable and capable of undergoing remarkable transformations.

Let us turn now to the question of the mode of origin of crystals, and the possible mode of origin of lowest organisms, so far as this is open to mere observation.

The question in each case is, whether by mere concurrence of certain physical conditions, aiding and abetting the inherent properties of the respective different material combinations, some kinds of matter can fall into modes of combination called 'crystalline,' whilst other kinds are capable of falling into modes of combination called 'living'; or whether, in each case, a pre-existing 'germ' of the particular kind of matter is necessary, in order to determine, in suitable media, either of these modes of combination. Are we to believe that crystals can appear in no solution whatsoever without the pre-existence in that solution of certain crystalline germs; and similarly that living things can arise in no solution whatsoever without the pre-existence in such solution of living germs?

The very mention of this question in connection with the origin of crystals may seem to some people to be quite absurd, because they may have been in the habit of believing that crystals could, and do, habitually come into being *de novo*, without the agency of pre-existing crystals. But in spite of the fact, that the majority of people are quite content to believe that crystals originate in obedience to purely physical conditions, and independently of pre-existing 'crystalline force'; still, facts somewhat similar to those which are to be met with in connection with the sister problem, long induced many chemists seriously to question the possibility of the *de novo* origination of crystals.

The way in which this problem was at last solved has been ably brought out by Prof. Léo Errera in an essay entitled "A propos de génération spontanée,"[1] an abstract of a portion of which I subjoin.

He points out that in 1865 Violette and Gernez demonstrated, independently, that crystallisation was brought about at ordinary

[1] "Revue de l'Université de Bruxelles," t. v., 1899-1900, Mai.

temperatures in supersaturated solutions of sodium sulphate by the access thereto of microscopic crystals of this salt, which exist (almost always) as constituents of atmospheric dust. These microscopic crystals, not much larger than the germs of microbes, act much like germs, and lead to the separation of crystals from the mother liquor.

Ostwald showed that salol, a white crystalline substance which melts at 39·5° C., may, when in this condition, be rubbed with a hair, a thread of glass or of platinum, or any angular substance, without inducing crystallisation. It will remain in the fluid state so long as it is not brought into contact with a crystal of salol; but directly it is touched with one of the previous objects after it has been brought into contact with salol, crystallisation is at once initiated in the fluid and spreads rapidly through its mass. The inoculating object, however, may be easily sterilised by raising it even momentarily to some point above 39·5° C. Very similar results were obtained with supersaturated solutions of hyposulphite of soda and chlorate of soda, which remained without change (sterile) until they were inoculated with microscopic crystals of these substances, of extreme minuteness.

"In face of such facts," says Errera, "it is natural to ask how the first crystal of each of these substances is born." And the reply, he says, must be, "by spontaneous generation."

Some supersaturated solutions may (when protected from the advent of all germs) be rendered fertile by allowing them to evaporate and become more concentrated, others by lowering the temperature of the solutions, and others still, even by slight mechanical shocks when the supersaturation is strong. De Coppet also made out the interesting fact that "other things being equal, the first crystal forms more quickly in large than in small masses of liquid," when dealt with in either of these ways.

In illustration of the effects of temperature some important investigations have been made with *bétol* by Tammann, of Dorpat. "Its point of fusion is more elevated than that of salol: it is 96° C. Melted at about 100°, and subsequently kept in little sealed tubes and cooled, it remains liquid for a period more or less long. But sooner or later, according to the temperature and the quantity of liquid employed, centres of crystallisation gradually appear, whence solidification is slowly diffused throughout the whole mass. For the same volume of liquid the number of its centres of crystallisation increases with the cooling, attains a maximum, and then decreases

pretty quickly. In the case of bétol the most favourable temperature—what in biological language would be called the 'optimum temperature' for the spontaneous generation of crystals—is about 10° C. But above and below this the phenomena progressively diminish, so that above 25°, or below −5° the bétol can be kept for a long time in the liquid state. . . . In addition, Tammann discovered that small quantities of foreign bodies, soluble or even insoluble (such as rock crystal or glass) suffice greatly to modify the number of germs—sometimes increasing and sometimes diminishing them."

Now comes a statement which is rather surprising: "But the temperatures most favourable for the generation of crystals are not the most favourable for their rapid growth; the optimum temperature for growth is notably higher than that for generation." Thus, expose tubes containing liquid bétol to about 10° for several minutes, and the fluid will still appear perfectly limpid, the newly-formed crystalline germs being so very minute as to be invisible. Now, as though dealing with a culture of microbes, expose these same tubes to an incubating temperature of about 20°, and in a few moments it will be seen that the crystalline germs have increased sufficiently to show themselves through the whole mass of the liquid." But it is important to note that if the tubes are exposed to this high temperature "without having been previously exposed to the low temperature favourable to the birth of germs, no crystallisation of bétol shows itself, even after a long trial."

The exact conditions under which, in liquid salol, a spontaneous generation of crystals becomes possible is unknown, although it is well known that crystallisation may be immediately induced in such a liquid by inoculating it with microscopic crystals of this substance at some temperature below 39·5° C. It is almost the same with glycerine. Up to 1867 this substance was only known in the fluid state, but then, after exposure to a low temperature and mechanical shocks, during a long railway journey from Vienna to England, a quantity of it was found to have become converted into a mass of white acicular crystals. This change to the crystalline state has been observed on a very few occasions since, but the conditions have not yet been accurately ascertained. It cannot be induced at will, though it can always be set up at once when the liquid glycerine is inoculated with a crystal of that substance; and, as with bétol, the process is rendered more rapid by heat up to a very moderate point. The heating must, however, only be slight,

because at about 18° C all crystals disappear, and the fluid would be sterilised.

Thus, from the point of view of crystallisation, there are two classes of fluids ; one (*a*) in which, under appropriate conditions, there may be a spontaneous generation of crystals, as well as a spread of the process induced by inoculation ; and another (*b*) where the latter process only seems possible and in which generation has never been known to occur. These two classes of fluids, of another order, have, of course, been long recognised also by those who believe in the "spontaneous generation" of microbes.

The 'germ' theory of the origin of crystals in supersaturated solutions has, therefore, not only been in existence, but has been overthrown. This has been possible, however, only because it has been more easy to show that a given set of conditions are inimical to the existence of a crystal, than it has yet been to induce people to believe that any given set of suitable experimental conditions, yielding positive results, are incompatible with the pre-existence of germs of living matter.

The analogy between the two problems as to the possible origin of crystals and organisms *de novo* in solutions, was rendered much more obvious by the discovery of the late Prof. Graham that, when dissolved, the saline substance does not remain as such in solution —but that the acid and the base exist separately, and are separable by a process of dialysis. When crystallisation occurs, therefore, we have a combination of molecules taking place similar to, though much simpler than, what may be presumed to take place in the genesis of a speck of living matter.

So far as evidence derived from microscopical examination can be adduced, however, it is able to speak no more decisively concerning the *de novo* origin of crystals, than concerning the *de novo* origin of organisms. In the elucidation of this point the valuable, though insufficiently known, observations of Geo. Rainey[1] come most opportunely to our aid. In ordinary cases, it is difficult to watch satisfactorily with the microscope the first stage in the appearance of crystals in solutions containing crystallisable matter, owing to the rapidity with which their growth takes place. This is one point in which crystals are strikingly different from organisms. The slower growth of organisms is, however, as Graham pointed out, quite in accordance with the general slowness

[1] "On the Mode of Formation of the Shell of Animals," etc. London, 1858, p. 9.

of colloidal changes. But, since Rainey's discovery that crystals are produced much more slowly, and undergo very important modifications in shape, when they are formed in viscid solutions, the formation of these bodies has, in both respects, become much more obviously comparable with that of organisms.

The appearance of these modified crystals may be best watched after mixing solutions of gum and carbonate of potash in the manner carefully described by Rainey. Owing to the viscid properties of gum, a solution of this substance diffuses with difficulty, and hence, when brought into contact with a solution of carbonate of potash, the malate of lime of the gum only decomposes very slowly. The insoluble carbonate of lime, instead of appearing in its usual crystalline condition, is precipitated in the form of globules resembling calculi. Rainey thus describes what takes place when portions of the two solutions are mixed under the microscope :—"The appearance which is first visible is a faint nebulosity at the line of union of the two solutions, showing that the particles of carbonate of lime, when they first come into existence, are too minute to admit of being distinguished individually by high microscopic powers. In a few hours exquisitely minute spherules, too small to allow of accurate measurement, can be seen in the nebulous part, a portion of which has disappeared, and is replaced by these spherical particles. Examined at a later period, dumb-bell-like bodies will have made their appearance, and with them elliptical particles of different degrees of excentricity." These modified crystals are, therefore, not produced much more rapidly than the lowest living things appear to be in other solutions during hot weather. The shapes of the products in the two cases, judging from Rainey's figures, are also remarkably similar ; and there is even a deceptive appearance of fission, produced by the juxtaposition of minute specks of carbonate of lime.

It will be interesting now to turn our attention to fluids containing organic matter in solution such as have been used in experiments on so-called 'spontaneous generation,' so as to ascertain what mere microscopical observation can tell us concerning the first appearance of living units therein.

The writer first made observations of this kind in 1871, using then a strong turnip infusion which had been filtered through several layers of the finest filtering paper. A drop of this fluid was filtered on to a thoroughly cleaned glass slip, over which a large

covering-glass was placed, and then a simple cement (proved to be harmless) was run round its edge leaving only a minute aperture—so as almost to prevent evaporation. The account given in "The Beginnings of Life" (Vol. I., p. 295) of the observations then made is as follows :—

"Mounted in the manner above mentioned, it is not difficult—with the stage of the microscope in a horizontal position—to bring into the field of view a portion of the film, which either contains no visible particles, or only a small number, such as can be easily counted.[1] With the slip resting on one of Stricker's hot-water plates maintained at a temperature of 85°–95° F., it may be found that, in the course of three or four hours, faint and ill-defined whitish specks, less than $\frac{1}{50000}''$ in diameter, make their appearance pretty evenly dispersed throughout the field of view. These are at first almost motionless—exhibiting only the merest vibrations, but no progressive movements. They gradually become more distinct, assume a sharper outline, and after a variable time some of them develop into distinct Bacteria. At first they exhibit gentle oscillations and tremblings only, though gradually they display the more characteristic darting movements. The study of the mode of origin of these primordial living forms is, indeed, facilitated and rendered much more certain by the fact that they remain comparatively motionless for a long time after their first appearance, and also continue faint and much less refractive than when in the more mature condition. Hence it becomes a matter of the greatest ease to watch their appearance in thin films of fluid, and also to distinguish them from other extraneous particles with which they may coexist."

I have recently adopted a simpler method for making observations of this kind, doing away with the necessity for any apparatus; and have used an infusion of turnip or one prepared from a small portion of fresh beef or mutton, which was subsequently filtered through two layers of the finest Swedish paper. A drop of one of these fluids is allowed to fall on a cleaned microscope slip, over which a $\frac{7}{8}$ of an inch square cover-glass is placed, and the excess of fluid is then removed by bringing a piece of blotting-paper to its edge. It is well to see that one or more minute air bubbles exist somewhere in the film before applying some melted paraffin wax round the edge of the cover-glass.[2] A small speck of paint

[1] Working with a magnifying power of 1,000 diameters.

[2] I have used wax which melts at 103° F.

may now be placed on the cover-glass just below one of the minute air bubbles, and the space between it and the bubble then carefully examined with the microscope. If the fluid in this region is found to be free from particles, that will be the region to be subsequently watched, and the edge of the air bubble will give the necessary guidance for bringing the film of fluid into focus.

The slip thus prepared may now be placed in an incubator, the temperature of which is maintained at about blood-heat ; and after three or four hours we may begin to make observations, replacing the slip in the incubator after each observation. The time necessary for the appearance of Bacteria will vary according to the nature and strength of the infusion used ; and of course will be more and more prolonged if lower temperatures are employed.[1]

But if, in a motionless film of fluid, multitudes of living particles subsequently appear, which are themselves almost motionless, how can we account for their origin ? Three hypotheses present themselves. It may be said (*a*) that they have arisen through the reproductive multiplication of one or more germs or organisms in the film of fluid which, though visible, had escaped observation. The difficulties standing in the way of our acceptance of this explanation are these. The film is motionless, and also those first appearing particles which gradually come into view in portions of it where no such particles had been previously visible. No multiplication by fission or other means can actually be observed to take place among the first appearing particles in question, though this ought to be easily observable if it really occurred at the rate postulated. And lastly, if the subsequent large numbers are to be accounted for by the occurrence of a reproductive process taking place among a few visible but unobserved germs, these products of fission, being at first motionless, ought to be aggregated here and there only, while as a matter of fact, this is not the case—the distribution of the particles being more uniform.

These various difficulties appearing fatal to this explanation of the mode of origin of the multitudes of plastide-particles and Bacteria, we are left with only two other possible modes of origin :—either (*b*) they have been developed from a multitude of diffusely disseminated *invisible* germs, or (*c*) they have been produced *de novo* in the fluid by a process of Archebiosis.

[1] Thus I have found that with a temperature as low as 85° F., and the film hermetically sealed, as in this mode of observation, the Bacteria usually begin to appear somewhere between the twelfth and the eighteenth hour.

Thus the solution of this great problem passes beyond the reach of actual observation. Microscopical evidence enables us to bring it to this stage now, and it may perhaps never enable us to do more. It reduces us to a consideration of two rival hypotheses, and to a careful consideration of whatever evidence may be forthcoming to influence us in our choice between these two possible explanations. Nothing that can be said about the abundance of recognisable atmospheric germs can directly affect the solution of this problem. It is one which, if it has to do with germs at all, has to do with invisible germs. But invisible germs can have only a hypothetical existence, and even to this they can lay no claim, unless observed phenomena cannot be explained without such postulation. We must not forget the old and well-approved logical rule—

'Entia non sunt multiplicanda præter necessitatem.'

The 'law of parsimony' may well be quoted for the benefit of those who would ruthlessly people the atmosphere with countless myriads of 'entities.'[1]

Let us now suppose however, for the sake of argument, that the living units which may be seen to make their appearance in the manner just described have, in reality, been formed *de novo* by a process of synthesis from the organic molecules contained in the infusion; and further, from the point of view of this hypothesis, let us consider what is known concerning the mutability of such forms of life, and whether—as should be the case if they are in fact new-born specks of living matter—they exhibit in an increased degree a capacity for change in form and nature such as we have found to be present in the case of crystalline matter.

We may suppose that Bacteria of all kinds (including Bacilli,

[1] Some of those who are so eager to demonstrate the prevalence of 'germs,' are frequently carried away, by their enthusiasm, beyond the bounds of strict logic. It suffices to show by the agency of the electric light or by some other means, that air and water contain myriads of infinitesimally small particles, some of which are organic in nature, in order that they may at once come to the conclusion that the organic particles are 'germs.' But, seeing the countless forms of life which exist upon the surface of the earth, and how these are from moment to moment, during life as well as after death, undergoing a molecular disintegration, it would be strange indeed if the atmosphere, and water which has been exposed to it, did not contain multitudes of organic particles, both large and small. The great majority of such mere organic particles, however, could have no reasonable title to be called "germs."

Micrococci, Staphylococci, Streptococci, etc.), and Torulæ merely represent the most prevalent forms which specks of new-born living matter are prone to assume;[1] and in conformity with this view, it may be stated that all intermediate shapes are frequently to be seen between the various forms, just as all intermediate conditions are to be seen between the smallest Bacteria of some highly fermentable infusion, and the larger Vibrio-like or Leptothrix forms which are frequently met with in other fluids.

The opinion was formerly held that Vibriones and Leptothrix are higher organisms than Bacteria, and that Torulæ are higher than either. Both these views, however, are equally devoid of any real foundation.

It is well known that the more rapidly crystalline matter separates from a solution, the greater is the number and the smaller the size and perfection of the crystals which appear. And similarly it may be found that the most fermentable solutions swarm rapidly with inconceivable numbers of small Bacteria; though if a drop or two of acetic acid has been added to another portion of the same infusion, it will not become turbid till many hours later, and the Bacteria which are present in the first scum may be much larger and such as are generally termed Vibriones or Leptothrix. In other cases, highly fermentable fluids which have been subjected to the influence of very high temperatures (270° F and upwards) will, even when exposed to the air, yield neither Bacteria nor Vibriones—though Torulæ will appear after a time, often as a sediment, and multiply more slowly.

Now with reference to such observations, the following considerations must be borne in mind. A highly fermentable solution is in one respect exactly comparable with a supersaturated saline solution. Both contain chemical elements which we may suppose to have a strong tendency to combine, and in both cases the products of combination are insoluble—particles of crystalline matter appear in the one case, and particles of living matter in the other. But the living matter differs essentially from the crystalline matter by reason of the complexity of its constituent molecules, and its consequent more marked instability. This capacity for free internal molecular movement, which is one of the most distinctive

[1] Under other conditions, of course, we may have the simplest Algæ appearing as non-nucleated, blue-green specks of protoplasm (the Chromacea); and under others still the simplest Amœbæ. All these forms, however, are just as varied and mutable as we shall find the Bacteria to be.

attributes of living matter, may well be marked in the products which separate from the most fermentable solutions; and it is precisely this attribute which is the principal factor in bringing about the self-multiplication, or 'discontinuous growth,' of living units.

Thus it is that rapid growth and rapid fission frequently go on simultaneously; so that although the total amount of living matter which separates from a solution may be large, the individual living units may be very small. Discontinuous growth is in excess, and therefore the fact of the growth being really rapid is apt to be overlooked.

All the differences in size and form recognisable between Micrococci and small Bacteria, between Bacteria and Bacilli or Spirilla, between the latter and Vibriones, whether jointed or unjointed; between Vibriones and Leptothrix filaments, plain or segmented in various ways; and between Leptothrix and the very delicate mycelial filaments of many Moulds, are easily explicable in accordance with these considerations. The several forms, many of which frequently occur together in the same solution, depend, in the main, upon the frequency with which segmentation tends to occur, and upon the degree of completeness of the process.

The forms of Torulæ, again, are almost infinite in variety as met with in different situations, and they are often notably different even in the same solution. They may vary in size from the minutest visible speck to a vesicle $\frac{1}{2000}''$ or more in diameter. They may after a time become spherical, ovoidal, ellipsoidal, or cylindrical in form, and produce buds in different directions.

It is impossible for us to assign any ultimate reason why one rather than the other of these forms should manifest itself. We can only observe that in some solutions different forms of Bacteria most frequently present themselves, while in others Torulæ are most prone to occur. It has been known, for instance, since the time of Dutrochet that the organic forms met with in acid and alkaline or neutral solutions vary; and it has been frequently observed by others, that Torulæ are most apt to present themselves in slightly acid solutions. Again, while the most putrescible solutions almost invariably yield Bacteria, the same fluids, after their fermentability has been greatly impaired by the influence of heat, may produce nothing but Torulæ. Torulæ are generally more frequent in saline solutions than Bacteria, and in some of these, after they have been boiled, no Bacteria ever present themselves.

Because in some solutions different forms of Bacteria invariably reproduce their like, just as in other solutions Torulæ reproduce Torulæ—because each of these forms 'breeds true'—they are commonly regarded and named as so many distinct 'species.' This, however, is far from being justifiable. A fragment which detaches itself from one of the lowest living things has just as much tendency to grow into the form of its parent, as the fragment detached from a given crystal has to reproduce a similar crystalline form. In each case, however, the parent form is reproduced only so long as the conditions remain the same. Placed under new conditions the crystalline fragment may grow up with a modified form ; and, similarly, under the influence of new conditions, a change may overtake a portion of matter thrown off from a pre-existing living form. In order that the crystal may lapse into another form, it seems necessary that the new conditions shall be capable of bringing about a new molecular arrangement (or isomeric state) of the crystalline matter. And, similarly, new conditions would probably change primordial living forms, only so long as they were capable of inducing internal molecular re-arrangements. It is, therefore, only to be expected that even new-born organic forms should remain constant, or 'breed true' so long as we have to do with the same fluids under unaltered conditions.

But since it is well known that different kinds of Bacteria and Torulæ may frequently be seen to grow in the same solution, we are compelled to believe that some minute difference in the constitution of their ultimate units exists, and that each has the power of causing, during its acts of growth, the synthesis of similar units of living matter. The occurrence of one or the other form need not, therefore, be always or wholly attributable to mere difference of 'conditions'—it ought to be mostly due to an actual, though minute, difference in the molecular constitution of the initial units of living matter. For although the same crystalline matter under the influence of different conditions may assume different crystalline forms, it is much more common for different crystallisable compounds to aggregate into different geometrical forms.

It must, moreover, be quite familiar to all who have had much experience in this particular line of research, that Torulæ frequently exist in abundance in certain solutions, and yet show no signs of developing into Fungi. Discontinuous growth goes on rather than continuous growth. So much is this the case, that

it was not till 1840 that the development of Torulæ into Fungi was traced.

The particular forms assumed by the outgrowths from germinating Torula corpuscles seem subject to much natural variation. Thus Trécul found that on exposure to the air the cells of beer yeast grew partly into the form of Mycoderma cerevisiæ, and partly into that of a large Penicillium. He became convinced, therefore, that the view originally advocated by Turpin is correct, viz. that Mycoderma and Penicillium are simply two forms which may be assumed by germinating beer Torulæ. Nay, more, the Mycoderma itself is observed to be most changeable in its form, as the qualities of the fluid in which it grows alter; and an already growing Mycoderma is said to be capable of taking on the mode of growth characteristic of Penicillium.[1]

Commonly, however, when beer-wort is exposed to the air and is not disturbed, Mycoderma begins to develop in about forty-eight hours—though curiously enough its appearance may be delayed for a fortnight or more by agitating the liquid two or three times daily. It commences in the form of the minutest specks, which gradually enlarge into ellipsoid corpuscles; these give birth after a time to a little bud at one extremity, and this grows into a corpuscle which in its turn produces another. Lateral buds are also produced, and after a time this mode of growth results in elegant, much-branched tufts, which vary in form as alterations in the medium proceed. But when beer-wort containing the Mycoderma is poured into a bottle (so as to fill it) and this is tightly stoppered, the plant ceases to grow in this form and gives place to an abundance of Torulæ—these being partly derived from portions of the pre-existing Mycoderma. The Torula form, and 'discontinuous' mode of growth, is that which seems to be invariably engendered when the liquid becomes more or less charged with CO_2 and alcohol, and when the pressure increases. Boiled beer-wort in a sealed vessel also produces Torulæ (see Note, p. 153) where there has been no pre-existing Mycoderma; and, according to Trécul, the Torulæ, thus engendered, after exposure to the air will also gradually assume the form of Mycoderma.

We have thus endeavoured to show that if, in accordance with

[1] See "Compt. Rend." t. 67, p. 1164. This development of Mycoderma into forms resembling Penicillium glaucum, was observed by Turpin in 1840, by the Rev. M. J. Berkeley in 1855 (from porter yeast), and subsequently, by Pouchet, in beer yeast and in that from cider.

our hypothesis, the Bacterium and the Torula corpuscle are only different modes of growth which may be assumed by new-born specks of living matter ; so also, as might be expected, are the varieties of each kind of growth both numerous and transitional. We have seen that each of the forms, under suitable conditions, may grow in a continuous rather than in a discontinuous fashion, the Torula then producing variously branched and articulated hyphæ, which at intervals are apt again to revert to the discontinuous mode of growth, so as to produce 'reproductive units'—either in single file as buds from a terminal expansion, or by segmentation of the contents of a terminal chamber. These, and many other simple variations in the mode of production of the reproductive units, variously combined with different sizes, modes of branching, articulation and segmentation of the filaments, go to produce the innumerable simpler kinds of Fungi or 'Moulds' which, like the innumerable varieties of Bacteria, instead of being lineal descendants of similar mutable organisms that lived in a pre-Adamite world, *may* be only different modes of growth continually being assumed by new-born living matter.

What is to be observed concerning the mutability of these low forms of life may, therefore, fairly be said to be quite consistent with the notion that they are, as they seem to be, capable of arising *de novo* in suitable fluids.

And if true, this would again be quite in harmony with what we have attempted to show in the previous chapters. We have seen how, under the influence of the well-established doctrine concerning the Persistence of Force—and more especially since the clear recognition of the subordinate doctrine as to the correlation existing between Physical and Vital Forces—the majority of biologists have long recognised that the phenomena manifested by living beings are to be ascribed simply to the properties of the matter as it exists in such living things. No one has expressed himself more decidedly on this subject than Prof. Huxley, and he may fairly be taken as an exponent of the modern doctrines on this question. In a striking article on "The Physical Basis of Life" he said[1] :—"But it will be observed that the existence of the matter of life depends on the pre-existence of certain compounds, namely, carbonic acid, water, and ammonia. Withdraw any one of these three from the world and all vital phenomena come to an end.

[1] "Fortnightly Review," Feby. 1869, p. 129.

They are related to the protoplasm of the plant, as the protoplasm of the plant is to that of the animal. Carbon, hydrogen, oxygen, and nitrogen are all lifeless bodies. Of these, carbon and oxygen unite in certain proportions and under certain conditions to give rise to carbonic acid; hydrogen and oxygen produce water; nitrogen and hydrogen give rise to ammonia. These new compounds, like the elementary bodies of which they are composed, are lifeless. But when they are brought together under certain conditions they give rise to the still more complex body, protoplasm; and this protoplasm exhibits the phenomena of life."

"I see no break in this series of steps in molecular complication, and I am unable to understand why the language which is applicable to any one term of the series may not be used to any of the others. We think fit to call different kinds of matter carbon, oxygen, hydrogen, and nitrogen, and to speak of the various powers and activities of these substances as the properties of the matter of which they are composed."

"When hydrogen and oxygen are mixed in certain proportions, and an electric spark is passed through them they disappear, and a quantity of water, equal in weight to the sum of their weights, appears in their place. There is not the slightest parity between the passive and active powers of the water and those of the oxygen and hydrogen which have given rise to it. At 32° F., and far below that temperature, oxygen and hydrogen are elastic gaseous bodies, whose particles tend to rush away from one another with great force. Water, at the same temperature, is a strong though brittle solid, whose particles tend to cohere into definite geometrical shapes, and sometimes build up frosty imitations of the most complex forms of vegetable foliage."

"Nevertheless we call these, and many other strange phenomena, the properties of the water, and we do not hesitate to believe that, in some way or another, they result from the properties of the component elements of the water. We do not assume that a something called 'aquosity' entered into and took possession of the oxide of hydrogen as soon as it was formed, and then guided the aqueous particles to their places in the facets of the crystal, or amongst the leaflets of the hoar-frost." . . .

"Is the case in any way changed when carbonic acid, water, and ammonia disappear, and in their place, *under the influence of pre-existing protoplasm*, an equivalent weight of the matter of life makes its appearance? . . .

"It is true that there is no sort of parity between the properties of the components and the properties of the resultant, but neither was there in the case of water. It is also true that what I have spoken of as the influence of pre-existing living matter is something quite unintelligible; but does anybody quite comprehend the *modus operandi* of an electric spark, which traverses a mixture of oxygen and hydrogen?"

"What justification is there, then, for the assumption of the existence in the living matter of a something which has no representative or correlative in the not living matter which gave rise to it?"[1]

[1] After this chapter was written the results of some experiments by J. B. Burke, in the Cavendish Laboratory at Cambridge, were published in 'Nature' (May 25, 1905), which attracted great attention, because they were supposed by some to have demonstrated the *de novo* origin of living units of a peculiar kind These the author proposed to term 'radiobes'—partly because they had been produced by the influence of radium on sterilised beef-gelatin, and partly because of his extraordinary suggestion that they have "probably arisen in some way from the invisible particles of radium." The very minute bodies produced are admitted not to be Bacteria, and have been supposed to be some other still more primitive form of life. That they have any claim to be living things seems, however, to be directly contra-indicated by the fact that they are soluble in water. The author's original notion that they were living things, seems to have rested almost solely upon the fact that the very minute particles not only grow, but, "*when they reach a certain size they sub-divide.*" As to this, one would like to ask Mr. Burke whether he has actually watched the process of sub-division, or whether he has assumed that it must occur because of appearances presented by his particles. He has said nothing to indicate that he has even watched the division. And that being so, let any one compare his figures with one of Rainey's figures which I reproduced in "The Beginnings of Life," vol. ii. p. 62, when it will be seen that the appearance of sub-division is just as exactly produced by the juxtaposition of minute crystals of carbonate of lime. Altogether apart from this, however, even if Burke had actually seen his particles divide, what would that show in face of the observations long ago made by Robin as to the spontaneous fissions exhibited by certain fatty extracts—an account of which I have quoted on p. 42? In the course of my investigations I have frequently encountered bodies as to whose nature it was difficult to decide (see "The Beginnings of Life," vol. ii., *Appendix* A). Concretions intermediate between crystals and organisms are, moreover, shown in that work in Fig. 43; while in Fig. 39 we have a crude imitation of cellular structure by modified crystalline aggregates—a subject which has recently been studied by Leduc in an article entitled "La Cytogenèse Expérimentale" ("La Nature," March 1, 1902).

CHAPTER IV

THE MOLECULAR CONSTITUTION OF LIVING MATTER AND ITS INNATE TENDENCY TO VARIATION

WHAT has hitherto been said goes to show that the properties of all bodies alike are due to their molecular composition and nature; and that in the case of compound substances their properties are wholly different from the properties of their constituent elements, and cannot even be guessed at from a knowledge of them.

The capacity for undergoing changes in appearance and properties under the influence of changing external conditions has been illustrated by a reference to some of the transformations that occur in different kinds of matter when aggregated into the forms of crystals and of lowest living units respectively. It was furthermore hinted that these changes and transformations could only have been induced by reason of the new physical influences having brought about some alteration in the molecular arrangement pre-existing in the several substances; and that, however easily such changes are capable of being induced in crystalline matter, they ought to be brought about still more easily in new-born living matter, by reason of its far greater molecular complexity.

We must look now a little more fully at this question of molecular composition in order, if possible, to acquire some vague notion as to the degree of its complicacy in living matter.

It may surprise some to know that molecular composition is an important item even with reference to substances that are looked upon as elementary—different modes of composition or arrangement of the atoms sufficing to produce in them what are called 'allotropic' states. We are most familiar with these as they are presented to us in the various forms of carbon. The differences between the diamond, graphite, anthracite, and pure charcoal are most striking, and yet these are all different states of one and the same substance whose ultimate atoms are differently grouped.

Oxygen exists in two different states—ordinary oxygen, and

ozone which is supposed to be represented by O_4. Sulphur crystallises in rhombic octahedrons belonging to the trimetric system, and also in rhombic prisms belonging to the monoclinic system. The latter have a deep yellow colour and are translucent, and always exhibit a great tendency to pass by molecular rearrangement—accompanied by an evolution of heat—into the opaque, straw-yellow, octahedral crystals.

There are, again, two varieties of phosphorus, known by the name of 'normal' and 'red Phosphorus.' The first variety is much more poisonous than the second; it is also colourless, crystallisable in rhomboid dodecahedra, soluble in sulphide of carbon, easily oxidisable, phosphorescent, and inflammable at a low temperature. The second form is chocolate or puce coloured, amorphous, much less soluble, non-phosphorescent, and not inflammable even at high temperatures. Lemoine has shown that heat is the most available means for converting the one form into the other, and that the transformation is always only partial.

Arsenic, antimony and other metals, also exist in allotropic states, and the two states of each exhibit wholly different properties.

These phenomena of allotropism show that even simple bodies—such as carbon, phosphorus, sulphur, and the metallic elements—are made up of molecules composed of similar atoms existing in varying though definite number and grouping in each allotropic state that may exist. An alteration of the number or grouping of the atoms in the molecules, or of both, seems indeed to be the only way of accounting for the wholly different properties and crystalline forms of one and the same substance, such as sulphur, under the influence of different physical conditions.

Thus vanishes a part of the difference between simple or elementary, and compound bodies. They are all made up of molecules; only those of the simple substances are aggregates of similar atoms, while those of compound substances are aggregates of dissimilar atoms.

Different compound substances vary, of course, immensely in their degree of molecular complexity. Some, such as ordinary acids or bases, are aggregates of simply complex molecules; others are aggregates of doubly complex molecules—that is to say, two simply complex molecules combine to form a doubly complex molecule, and these include, among other compounds, the very common class of bodies known as 'salts.' Seeing that in bodies of

this class a compound radicle, such as cyanogen (CN) or ammonium (NH_4), may replace one of the simple metallic elements, and that two such salts may combine together to constitute a double salt; or that the metallic element may be replaced by a more complex radicle, such as urea ($C^2N^2H^4O^2$), or kreatinine ($C_8H_7N_3O_2$), or even by one of the still more complex bodies known as alkaloids,[1] we may be somewhat amazed at the marvellous atomic complexity which is to be attained even by the crystallisable bodies known as salts.

What has been said concerning crystallisable bodies obtains also with regard to the compounds known as 'colloids.' In this class are included all the plastic elements of animals and plants—that is, the various protein compounds. They are supposed to be generally characterised by the large size and complexity of the molecules of which they are compounded. Thus Prof. Graham says[2] :—" It is difficult to avoid associating the inertness of colloids with their high equivalents, particularly where the high number appears to be attained by the repetition of a smaller number. The enquiry suggests itself whether the colloid molecule may not be constituted by the grouping together of a number of smaller crystalloid molecules, and whether the basis of colloidality may not really be this composite character of the molecule."

No hard and fast line, however, separates the colloids from the crystalloids. Although multitudes of bodies exist which may be easily placed in one or the other class, multitudes of others are to be met with having properties of an altogether intermediate character. Nay, even the most typical colloids may undergo a rearrangement of their elements, whereby they are converted into crystalloids. Nothing could show more plainly than this, that the difference between a crystalloid and a colloid is merely one of degree, and that the properties of colloids are different merely by reason of the more complex molecular arrangement which prevails—an arrangement, however, which, by the mere influence of physical conditions, the molecules of certain crystalloids are known 'spontaneously' to assume. A further consequence directly flowing from this superior complexity of the colloid molecules, is, the greater instability which characterises them as a class.

[1] The composition of narcotine, for instance, is said to be $C_{48}H_{25}NO^{14}$, and that of morphine, $C_{34}H^{19}NO_6 + 2HO$. Both bodies have distinct crystalline forms.

[2] Phil. Trans., 1861.

Very slight changes in the conditions or influences to which the colloid is exposed lead to changes in its constitution—owing to the ease with which a re-arrangement is brought about among its constituent atoms or elementary molecules. Its very existence, as Graham pointed out, is one of "continual metastasis."

It is owing to the great size and complexity of the molecules of which they are composed that colloids, in their ordinary state, will not pass through a parchment membrane. Their molecules are too big to be able to get through its pores; though various saline substances will pass through with the greatest ease, and thus can be easily separated by dialysis from the colloids with which they may have been intermixed.

But, under the influence of certain re-agents, more especially the digestive juices of the stomach and pancreas, proteids undergo most important modifications which enable them, like crystalloids, to diffuse through membranes. When thus modified they are known as 'peptones,' and in regard to these bodies Verworn says[1]:—"It is known that they arise by the hydrolytic cleavage of the original proteid molecule, so that the peptones represent the hydrates of the original proteids. Important conclusions follow from this fact. Since the proteid molecule, which was originally not diffusible on account of its enormous size, is split up in the peptonising process into the peptone molecules, which are much smaller and therefore diffusible, but which have the chemical characteristics of proteids, it follows that the proteid molecule is not simple but polymeric, *i.e.*, it consists of a chain-like combination of many similar groups of atoms. In the transition to the peptone condition the proteid molecule is broken up with hydration into these single, similar atomic groups, all of which, however, have the chemical characteristics of proteids, but represent much smaller molecules."

Let us look at these compounds now from another point of view.

Before Wöhler announced to the scientific world that he had succeeded in building up an organic compound in his laboratory with the aid of no more mysterious agencies than usually lie at the chemist's disposal, and before the labours of other distinguished chemists had been crowned, with a like success, there was more reason than there is at present for the belief that the forces in living things are altogether peculiar, because it was

[1] "General Physiology," Transln. 1899, p. 106.

thought that certain compounds of carbon with other elements, known as 'organic' substances, were capable of being produced only within these laboratories of nature. A department of 'inorganic chemistry' had hitherto existed, separated quite definitely from another known as 'organic chemistry.' In the former were included all those elements and their compounds that were naturally met with among, and which made up, the not-living constituents of our globe ; while under the latter department were ranged such compounds as proteids, carbohydrates, fats and their derivatives which were supposed to be produced only in plants and animals.

The so-called organic compounds were for a long time regarded as altogether peculiar ; not as regards their components—for they were known to be composed of precisely the same elements as were most abundant in the inorganic world—but rather in point of origin. *They* were the products only of living things—had been produced under the influence of 'vital' forces. The action of physical forces in the world without was deemed inadequate to give rise to such combinations ; and therefore they were separated by a hard and fast line from all other compounds with which the chemist manipulated. Thus the popular belief of the time concerning 'life' was fostered ; and an argument for the special and peculiar nature of the 'vital forces,' could, at least, be based on the supposed fact that living things produced substances—were in fact almost entirely composed of material combinations—which could not be evolved by the agency of mere physical forces, either in the grand laboratory of nature, or under the hands of the chemist. But now all this has changed. Chemists have already succeeded in building up many hundreds of such compounds, and, as each month passes, the list is swelled by fresh conquests. The speciality then of these compounds has passed away ; the difference between organic and inorganic chemistry is fast vanishing—has, in fact, well-nigh vanished.

Members of this great group of albuminous or proteid compounds always contain, as principal and fundamental ingredients, carbon, oxygen, hydrogen, and nitrogen, and to these are commonly added traces of sulphur and phosphorus. The first four elements are, however, all-essential, and it is especially worthy of remark that no less than three of them are gaseous. Herbert Spencer says[1] :—"When we remember how these re-distributions of

[1] "Principles of Biology," vol. i., chap. i., 'Organic Matter.'

Matter and Motion which constitute Evolution, structural and functional, imply motions in the units that are redistributed; we shall see a probable meaning in the fact that organic bodies which exhibit the phenomena of Evolution in so high a degree, are mainly composed of ultimate units having extreme mobility." When such mobile units enter into various combinations, this initial property, though masked, is still potentially present, and must have its influence upon the molecular mobility of the compounds into which they enter. Hence Herbert Spencer adds, "We may infer some relation between the gaseous form of three out of the four chief organic elements, and that comparative readiness to undergo those changes in the arrangement of parts which we call development, and those transformations of motion which we call function. . . . One more fact that is here of great interest for us must be set down. These four elements of which organisms are almost wholly composed, present us with certain extreme antitheses. While between two of them we have an unsurpassed contrast in chemical activity; between one of them and the other three we have an unsurpassed contrast in molecular mobility. While carbon by successfully resisting fusion and volatilisation at the highest temperatures that can be produced, shows us a degree of atomic cohesion greater than that of any other known element, hydrogen, oxygen, and nitrogen show the least atomic cohesion of all elements. And while oxygen displays, alike in the range and intensity of its affinities, a chemical energy exceeding that of any other substance (unless fluorine be considered an exception), nitrogen displays the greatest chemical inactivity.[1] Now on calling to mind one of the general truths arrived at when analysing the process of Evolution in general, the probable significance of this double difference will be seen. It was shown ('First Principles,' § 123) that, other things equal, unlike units are more easily separated by incident forces than like units are—that an incident force falling on units that are but little dissimilar does not readily segregate them, but that it readily segregates them if they

[1] Hence its compounds are generally most unstable. "Here it will be well to note, as having a bearing on what is to follow, how characteristic of most nitrogeneous compounds is this special instability. In all the familiar cases of sudden and violent decomposition, the change is due to the presence of nitrogen. The explosion of gunpowder results from the readiness with which nitrogen contained in the nitrate of potash yields up the oxygen combined with it. The explosion of gun-cotton, which also contains nitric acid, is a substantially parallel phenomenon."

are widely dissimilar. Thus, these two extreme contrasts, the one between physical mobilities, and the other between chemical activities, fulfil in the highest degree a certain further condition to facility of differentiation and integration."

The very fact, then, that organisable matter is, in the main, compounded of elements with such dissimilar properties, affords a strong *à priori* presumption that such organisable matter would be most unstable, and most prone to undergo metamorphic changes under the influence of even slight changes of condition—such as might operate without appreciable result upon the majority of inorganic substances. The properties of the various proteid substances which form the all-essential constituents of living tissues, are found to correspond entirely with these *à priori* requirements. This can scarcely be better shown than it has been by H. Spencer when he wrote[1] :—" It is, however, the nitrogenous constituents of living tissues that display most markedly those characteristics of which we have been tracing the growth. Albumen, fibrin, casein, and their allies are bodies in which that molecular mobility exhibited by three of their components in so high a degree is reduced to a minimum. . . . It should be noted, too, of these bodies, that though they exhibit in the lowest degree that kind of molecular mobility which implies facile vibrations of the atoms as wholes, they exhibit in a high degree that kind of molecular mobility resulting in isomerism, which implies permanent changes in the positions of adjacent atoms with respect to each other. . . . In these most unstable and inert organic compounds, we find that the atomic complexity reaches a maximum : not only since the four chief organic elements are here united with small proportions of sulphur and phosphorus, but also since they are united in high multiples. The peculiarity which we found characterised even binary compounds of the organic elements, that their atoms are formed not of single equivalents of each component, but of two, three, four, and more equivalents, is carried to the greatest extreme in these compounds that take the leading part in organic actions. According to Mulder, the formula of albumen is $10(C_{40}H_{31}N_5O_{12})+S_2P$. That is to say, with the sulphur and phosphorus there are united ten equivalents of a compound atom —containing forty atoms of carbon, thirty-one of hydrogen, five of nitrogen, and twelve of oxygen : the atom being thus made up of nearly nine hundred ultimate atoms."

[1] *Loc. cit.* p. 12.

More recent analyses tend to show that the molecular constitution of some proteids is, however, even much more complex than that just given. Thus hæmoglobin is a proteid in combination with iron, whose presence in the blood corpuscles gives its characteristic colour to this fluid. It has several times been analysed by skilled chemists, and though their results have differed a good deal in details they have all shown it to be a compound of extreme complexity. Pryer, for instance, found the composition of hæmoglobin to be as follows—

$$C_{600}H_{960}N_{154}Fe_1S_3O_{179}.$$

On the other hand, it seems equally clear from what has previously been said (p. 18) concerning the free growth of Bacteria and Torulæ in solutions of ammonic tartrate in distilled water, that proteids and even protoplasm can be formed readily enough from C, H, O and N, together with the merest trace of sulphur or phosphorus, which are commonly regarded as necessary and invariable elements in a proteid compound.[1]

Most of these compounds whose molecules are so very complex, are known to be capable of existing under very many different isomeric modifications. Protein, for instance, according to Prof. Frankland, is capable of existing under probably at least a thousand isomeric forms; and this, as we have seen, is the substance which, in one state or another, enters so largely into the fabric of living things as to be, above all else, *the* organisable material—the molecules of which are, in some at present unknown way, built up so as to form the still more complex body, protoplasm. The possibilities of change and isomeric variation in this more complex product, are further enormously increased by the common presence of additional elements, which must be taken up by the proteids and are thus brought into its composition. These common additional elements found in protoplasm are chlorine, potassium, sodium, calcium, iron, and magnesium.

Three groups of proteids, varying in their solubility in water, are known—as 'albumins,' 'globulins,' and 'vitellins'; while in addition there are many very important bodies known as 'albuminoids,' which are derivatives, differing from the genuine proteids

[1] Sir William Ramsay has been kind enough to analyse one of the solutions of ammonic tartrate for me, and he reports that the "liquid contained an excessively minute trace of sulphur, probably as sulphate; but no phosphoric acid could be detected by the molybdate of ammonium test."

and yet in many respects like them. They include various horny and elastic substances found in skeletal tissues and in skin developments; and also the extremely important group of 'enzymes' or soluble ferments to which reference was made in the last chapter—including ptyalin, pepsin, trypsin, pancreatin, etc.

Multitudes of other organic compounds exist as decomposition products of the proteids—results of disassimilation. Many of these are thrown off as waste products, the commonest of all being carbonic dioxide and urea; others, such as 'fats' and 'carbohydrates,' are very important non-nitrogenous compounds, the latter including different kinds of sugar and starch met with principally in plants and of great importance in their economy.

Although such a radical distinction exists between crystalloids and colloids, in regard to their intimate molecular constitution, as to have led Graham to say, "Every physical and chemical property is characteristically modified in each class," so that "they appear like different worlds of matter," yet the transition from the one state to the other seems to be comparatively easily brought about. As Graham has shown, one and the same saline substance may exist with its molecules now in the crystalloid and now in the colloidal mode of aggregation—according to the different influences to which it has been subjected—or under which it has been produced. This, for instance, is the case with silica, with the sesquioxides of chromium and iron, and with other mineral substances. Nay, more, the absence of any natural barrier between the crystalloid and the colloidal mode of aggregation may be still further seen by the fact that even the most typical colloids are capable of undergoing that kind of isomeric molecular change which converts them into crystalloids. As one of the best instances of this we may mention the fact of the change which blood pigment undergoes. Hæmatoidin is frequently met with in the form of oblique rhombic crystals, and in addition there are other crystalline forms of albuminoid substances obtainable from blood,[1] and also from the cells of some plants (known as aleurone-granules). Chlorophyll has likewise been observed in a crystalline state by Trécul.[2] While Marcet,[3] as a conclusion drawn from certain experimental researches expressed the opinion, "That there

[1] See an article on "Albuminous Crystallisation," in 'Brit. and For. Med. Chir. Review,' Oct. 1853.
[2] 'Comptes Rendus,' t. LXI., p. 436.
[3] 'Proceed. of Roy. Soc.,' Vol. XIX. (1871), p. 455.

is a constant change, as rotation in nature, from crystalloids to colloids, and from colloids to crystalloids."

These facts sufficiently show that, notwithstanding all their differences in property, the transition is fairly easy from the one to the other isomeric state. And if simple saline substances can pass into the colloidal condition, it surely should not be difficult to imagine that molecular rearrangements may take place among the constituents of ammoniacal salts of greater complexity, whereby a colloid may be produced capable of entering into the formation of that simplest form of protoplasm, such as must be engendered by the Bacteria that grow and multiply, as I have shown, in a simple solution of tartrate of ammonia in distilled water. The simplicity of their life-processes must exceed that of the nitrifying Bacteria, just as this in its turn exceeds that of the simplest Algæ (the Chromacea): though in each case their protoplasm is directly fashioned by the synthesis of inorganic elements.

If the living unit—the unit of protoplasm—is itself constituted, as is commonly supposed, by a grouping of colloid molecules, we are introduced to a substance whose molecular diversity may well be as endless as that of the countless simple forms of life, together with the similar variety of tissue elements of which plants and animals are constituted. In its simplest form protoplasm exists as a jelly-like, slightly granular, semi-fluid substance, without visible structure, though it is presumed to have an excessively minute sponge-like or foam-like character; and to be capable of absorbing dissolved matter from without, as a preliminary process leading on to assimilation and growth. But as Haeckel says,[1] "We have as yet no knowledge of the fundamental features of its very variable chemical structure. The one thing that bio-chemists have told us about it is that the molecule of plasm is very large, and made up of a great number of atoms (over a thousand); and that these are combined in smaller or larger groups, and are in a state of very unstable equilibrium, so that the life process itself causes constant changes in them."

This then is the material that was spoken of by Huxley as 'The Physical Basis of Life'; and the upholders of the Protoplasm or Sarcode theory maintain that this substance has an essential unity of nature. So that, in spite of multitudes of minute specific and isomeric differences, we have in reality to do with one and the

[1] "The Wonders of Life," 1904, Transln. p. 140.

same generic substance, whether existing as the 'contents' of animal and vegetable cells, or as naked masses of protoplasm—whether as parts of higher organisms, or as single independent living units. The recognition that all these various forms are but trifling alterations of a single great genus of primitive organisable material, and that in all cases this "albuminous material is the original active substratum of all vital phenomena, may," says Prof. Haeckel, "perhaps be considered one of the greatest achievements of modern biology, and one of the richest in results."

Protoplasm then, in its most general and undifferentiated condition, in the form of a naked contractile mass, of seemingly homogeneous, semi-fluid jelly, is the variable substratum for all the life-movements of the lowest living things, even in their adult condition. A minute structureless mass of jelly, now in one now in another isomeric state, suffices for the display of all the vital phenomena of the lowest organisms. Here, without the aid of organs of any kind, are carried on the vital phenomena of 'growth' and 'reproduction'; here do we see the first germs of that organic irritability and contractility which attain their highest development in the conscious sensibility and power of movement possessed by those living things that stand at the head rather than at the foot of organic nature.

CHAPTER V

THE 'IDS' AND 'DETERMINANTS' OF WEISMANN *VERSUS* THE 'PHYSIOLOGICAL UNITS' OF HERBERT SPENCER

THE general acceptance of the Doctrine of Evolution initiated by the publication of Darwin's epoch-making work, "The Origin of Species," soon attracted enormous attention to the problems connected with Heredity; and some of these doctrines in their turn have sufficed to focus the labours of a vast number of workers upon the structure of cells, and upon the excessively complicated processes commonly taking place in such bodies when they divide, and more especially upon the processes occurring in those particular cells—the germ cells and sperm cells—upon whose union the development of different animals and plants for the most part depends.

The result has been a vast amount of scientific literature dealing, both theoretically and experimentally, with the subject of Heredity,[1] and one equally vast on the subject of Cytology—that is concerning the structure, functions, and life history of cells. To some of these discussions a brief reference must now be made.

What has already been said in previous chapters should have made it perfectly clear, that we must look for the fundamental properties of living matter as being possessed by certain chemical units of great complexity, though so minute as to lie altogether beyond the range of any existing microscopical powers; and, further, that such invisible units when they grow, by the continuance of synthetic chemical processes, result in the appearance of minute particles of protoplasm which, though structureless, speedily assume one or other of the well-known forms of lowest organisms. We have, moreover, seen reason to believe that such

[1] The modern discussions on this subject may be said to have been initiated by Darwin's hypothesis of "Pangenesis," published in 'Animals and Plants under Domestication," 1868, Vol. II., p. 357. An improved form of this rather crude hypothesis has since been put forward by Hugo de Vries, in his "Intrazelluläre Pangenesis," Jena, 1889.

forms may be represented by the various kinds of Bacteria, of Chromacea, or of simplest structureless Amœbæ.

It must be clear from the point of view of Evolution that Unicellular Organisms and the various developed forms of Cells entering into the structure of multicellular plants and animals, are products which have been evolved from structureless units of living matter like one or other of the above-mentioned, lowest-known forms of life, or units as simple as these.

We have seen how freely the process of fission is capable of taking place in all the lowest forms, in which no trace of a nucleus exists; how it occurs also in non-nucleated, filamentous Algæ like Vaucheria, and even in chlorophyll corpuscles—showing clearly that this rudimentary 'reproductive process' is in them essentially dependent upon the molecular properties of the living matter itself, and that it is not in relation with any visible structure. These are facts only too much disregarded by many of those who base their theories of Heredity upon what goes on in cells, and by others in their attempts to establish a causal relation between some of the processes occurring in an elaborately developed cell while it is dividing, and going through all the mysterious changes included under the term 'karyokinesis.' Formerly the nucleus, for instance, was supposed to be the determining cause of this whole process, but since the discovery in 1885 by Van Beneden of another product of evolution in the cell, whose origin is still more mysterious, namely, the "centrosome" the tendency has been to look upon this minute body as the primary directing agent, in the various processes occurring during the division of highly developed cells—the changes in the nucleus and other parts occurring, as it were, at its instigation. Whatever the interpretation of the relation between the several processes may be, no one can study them without being impressed by their truly wonderful nature, and without recognising the complicacy of these processes of molecular physics which take place during the life and multiplication of cells—bodies that were formerly, though wrongly, regarded as the simplest of vital units.

What has just been said may give a clue to the reason why there are now two distinct biological schools in reference to the problems connected with Heredity, and those developmental tendencies of living matter that lie at the root of Evolution—tendencies which, with other causes, have eventuated in the production of all the forms of life that now exist, or in past ages have ever appeared, upon the earth. There are those, for instance, who start in their

conceptions concerning these processes, (*a*) from a mere *matter* of life, and regard the simplest vital units as composed of more or less complex aggregates of chemical molecules, whether they have been spoken of as "physiological units" by Herbert Spencer (1863), as "plastidules" by Haeckel (1875), as "micella" by Nägeli (1884), as "pangenes" by de Vries (1889), or as "biogens" by Verworn (1894). There are others, however, who, while professed evolutionists, base their theories upon (*b*) a particular *form* of life, the nucleus, and make that, which must be a product of Evolution, the starting point for their theories. These latter views are more especially associated with the name of Weismann (1881–1904) and his gradually developed hypotheses concerning 'ids' and 'determinants' as constituents of the nucleus.[1]

It will be best for us first to say something concerning these views of Weismann. They are, however, so complicated as to make it difficult to give any adequate account of them in a few words. They can count upon very many firm adherents; though others are more struck with the number of unproved assumptions and more or less baseless hypotheses which are most skilfully used with the appearance of explaining this or that problem as it arises. Some of Weismann's fundamental positions, however, are so absolutely hypothetical and incapable of verification, if not actually opposed to existing knowledge, as necessarily to instil the gravest doubts as to the value of his theories. They lead him to attribute to 'natural selection' together with his supplementary doctrine of 'germinal selection' (as causes of Evolution) an influence much more profound and far-reaching than was ever claimed by Darwin himself: they lead him to deny the evolutional effects of "use and disuse," and, in other words, the "inheritance of acquired characters"—processes which are regarded by Herbert Spencer, Haeckel and others as real and very important factors in the process of Evolution.

Weismann postulates the presence in living matter of a special *hereditary substance*, and this substance he believes to exist in the 'chromatin granules' which, when aggregated, form the 'chromosomes' of the nucleus, as they occur in the germ-cells and sperm-cells, as well as in cells generally. These chromosomes of the nucleus are often band or rod-shaped, more rarely minute and granular, though in the former case the bands or rods are

[1] The latest presentation of his doctrines is to be found in a recently published work, "The Evolution Theory," 1904. (Transln.) Two Vols.

made up of aggregates of granules—the granules in each case being what Weismann calls 'ids.'[1] The number of 'chromosomes' varies much in different kinds of animals and plants though it "remains the same in every cell-generation throughout development, as it is the same in all the individuals of a species." Thus, according to Weismann, "in some worms there are only two or four; in the grasshopper there are twelve; in the mouse, the trout, and the lily there are twenty-four; in snails thirty-two," while in Man "the chromosomes are so small that their normal number is not certain—sixteen have been counted" (*loc. cit.* I, p. 291).

After these well-established facts we come into the region of pure hypothesis. The minute granular 'ids' are assumed to be composed of a vast number of altogether hypothetical units to which Weismann gives the name of 'determinants,' though he also speaks of them as 'primary constituents.' Each 'id,' of which there are usually very many in each chromosome, is represented to be a "complex of primary constituents necessary to the production of a complete individual," or, as he says a little further on, "as composed of a mass of different kinds of parts each of which bears a relation to a particular part of the perfect animal, and so to some extent represents its 'primary constituents' (Anlagen), although there may be no resemblance between these 'primary constituents' and the finished parts." There must, he says, be as many of these 'primary constituents' or 'determinants' "as there are regions in the fully formed organism capable of independent and transmissible variation, including all the stages of development,"—even where these latter are so very different as they are found to be in many crustacea and in insects.

Thus, according to these hypotheses of Weismann, each granule of one of the minute chromosomes existing in a nucleus (that is each 'id') must contain all the infinitely numerous entities, known as 'determinants,' which are assumed to be necessary for the development, let us say, in a butterfly, of the larva, the pupa, and the imago; and in the latter for the myriads of every minute part or scale of the butterfly's wing; since, as he says, "the primitive cell of the butterfly's wing must contain all the determinants for the building up of this complicated part." Divisions of the cells will continue, he adds, "until the determinant archi-

[1] This abstract of his views is gathered from Lects. XVII., XVIII., and XIX. on "The Germ-Plasm Theory," pp. 345–416.

tecture of the ids is completely analysed or segregated out, so that each cell ultimately contains only one kind of determinant, the one by which its own peculiar character is determined" (p. 378).

But how, it may be asked, are the determinants always to find themselves in the right place? According to Weismann each of them "must be guided through the numerous cell-divisions of ontogemy so that it shall ultimately come to lie in the cell which it is to control. . . . This presupposes that each determinant has from the very beginning its definite position in relation to the rest, and that the germ-plasm therefore is not a mere loose aggregate of determinants, but that it possesses a structure, an architecture, in which the individual determinants have each their definite place"—their relative positions depending, as he says, partly upon heredity and partly upon internal forces or "vital affinities."[1] But if "the determinants must separate from each other in the course of development, so as to penetrate singly into the cells they are to control," the 'id' must have the power of dividing into daughter 'ids' of identical and also of dissimilar halves. This second mode, or "differential division," is not a process that can be observed. Its existence is one of the numerous assumptions made by Weismann in support of his theories, in order to attempt to account for things otherwise inexplicable. Thus, he says, when "one of two sister cells of the embryo gives rise to the cells of the alimentary canal and the other to those of the skin and the nervous system, I infer that the mother cell divided its nuclear substance in a differential way." Yet what has been proved to occur in the ova of the Sea-urchin, of the Amphioxus, and in those of some other animals is directly opposed to this supposition, and shows that their blastomeres are identical in composition. It is not surprising, therefore, that this supposition of a "differential nuclear division" has not been favourably received, especially seeing that Weismann has to make a further postulation of the existence of forces or "affinities" within the ids which control and regulate these identical or differential divisions.

Speaking generally Weismann holds "that the determinants

[1] Though Weismann uses the term "germ-plasm" as that which possesses what he calls "an architecture," it should be observed that here, as in other places, he falls back upon the more imposing term "germ-plasm" when he is really speaking of a mere minute granule—one of his 'ids.'

gain entrance into the cells they are to control, by a regulated splitting up of the ids into ever smaller groups of determinants, by a gradual analysis or segregation of the germ-plasm into the idio-plasms of the different ontogenetic stages." But, he says, in accordance with this view "it must strike us as remarkable that the chromatin mass of the nucleus does not become notably smaller in the course of ontogeny, and even ultimately sink to invisibility. Determinants lie far below the limits of visibility, and if there were really only a single determinant to control each cell there would be no chromatin visible in such a case." But, ever ready to meet such a difficulty, Weismann does not hesitate to make a further postulation. He accordingly assumes "that in proportion as the number of the *kinds* of determinants lying within a cell diminishes, the number of resting determinants of each kind increases. [Why this should be so, is not said.] When, finally, only one kind of determinate is present there is a whole army of determinants of that kind"—but even then they are assumed to exist in "two different states, at least in regard to their effect upon the cell in which they lie: an active state in which they control the cell, and a passive state, in which they exert no influence upon the cell, although they multiply." All this again is pure hypothesis: and now come further hypotheses, equally devoid of foundation in fact.

We have the determinants gradually "segregated" in the course of development, so that each finds itself, we must not say in different kinds of cells, but rather in cells differently situated, because the determinant itself is supposed to be the main influence in producing the different kinds of cells. The question then arises, however, as to the mode of action of the determinants, and this is what Weismann says:—"It seems to me that the determinants must ultimately break up into the smallest vital elements of which they are composed, the biophors,[1] and that these migrate through the nuclear membrane into the cell-substance. But there a struggle for food and space must take place between the protoplasmic elements already present and the new-comers, and this gives rise to a more or less marked modification of the cell-structure." It is for this end, therefore, and with a view to the results of this "struggle for food and

[1] These 'biophors' have not been mentioned before, but they are comparable with the elementary vital units of Haeckel (plastidules), or those of Verworn biogens), which are referred to on p. 75.

space" that all the apparatus of determinants exist, and that all the provisions are supposed to be made with the aid of special "vital affinities," for the proper "architecture" of the granular ids, and for the fitting "segregation" of determinants throughout all the stages of development of the particular animal or plant!

We might go on enumerating several other requirements that would have to be taken into account, relating, for instance, to double sexual determinants; to the requirements in the case of the dimorphism of larvæ; and the polymorphism often existing in such social insects as ants and bees (See *loc. cit.*, Vol. I, p. 390). Then, again, as each id is presumed to consist of a complex of determinants necessary for the production of a complete individual, as there are many ids in each chromosome and many chromosomes in each nucleus, it follows that there must be a whole host of determinants which would have to find their way into each cell during development, so that very special 'forces' would need to be called into operation to make the multitude lie low while some chosen determinant (through the 'struggles' of its liberated biophors) was impressing its special characters upon the previously indifferent embryonal cell. And, looking to this work that the biophors are supposed to perform, it seems clear that there must be a further complication: they must be endowed with mysterious specific qualities. Thus Weismann says, "in any case the biophors which transform the general character of the embryonic cells into the specific character of a particular tissue-cell must themselves possess a specific structure different from that of other biophors, for they must keep up the continuity of structures handed on from ancestors—chlorophyll, and muscle-substance, and the like."

To be asked to accept anything of this kind as a rational interpretation of what occurs in the development of an individual, must be felt by many to make serious demands upon their faith; and this strain may give place to actual incredulity when they learn that this whole agglomeration of assumptions and hypotheses has been built up upon a doctrine which is itself, to say the least, devoid of any firm foundation in fact, and may fairly enough be said to be strongly opposed to many well-known facts —I allude to the author's celebrated and fundamental doctrine as to "the continuity of germ-plasm," and the "immortality of germ cells" which he has advocated since 1885.

He would have us believe that the ordinary cells of the body—the 'somatic cells'—are always quite distinct from the 'repro-

ductive cells'; that there is a strict continuity of the latter from one generation to another, and that they never take origin afresh, in the course of an individual's development, as liver cells, kidney cells, or brain cells are known to do.

But what are the facts? It is a matter of common knowledge, and admitted by Weismann himself, that the fertilised ovum segments into hundreds, "thousands or even millions of cells" before, in some animals, the primary germ-cell appears in the rudiments of the ovary.[1] And speaking of this latter he says in his last work ("The Evolution Theory," I, p. 300):—"We find that it consists of a single primitive egg-cell, from which, by division, all the other egg-cells arise. In the same way the first rudiment in the testis or spermary is formed by a primitive sperm-cell which does not differ visibly from the primitive egg-cell. Both now multiply by division for a considerable time." Where then, it may be asked, is the alleged continuity of the germ-plasm? Only another "assumption" is needed to bring this about. Thus further on (p. 410) he says the 'primordial germ-cell' makes its appearance, "usually during embryogenesis, and often very early in it, after the first few divisions of the ovum, but sometimes not till long after the end of embryogenesis; and not even in the individual which arises from the ovum, but in descendants which arise from it by budding. Here the primordial germ-cell is separated from the ovum by a long series of cell-generations, and the sole possibility of explaining the presence of germ-plasm in this primordial cell is to be found in the *assumption*[2] that in the divisions of the ovum the whole of the germ-plasm originally contained in it was not broken up into determinant groups, but that a part, perhaps the greater part, was handed on in a latent state from cell to cell, till sooner or later it reached a cell which it stamped [why that rather than any previous cell?] as the primordial germ-cell. Theoretically it makes no difference whether these 'germ-tracks,' that is, the cell-generations which lead from the ovum to the primordial germ-cell, are short or very long, whether they consist of three or six or sixteen cells, or of hundreds and thousands of cells." As to the reality of the existence of such 'germ-tracks' not a tittle of evidence is given, or even attempted. Weismann's ever-ready assumptions cannot be accepted as proofs—so that many of us can only regard his doctrine of the "continuity of the germ-plasm"

[1] "The Germ Plasm," 1895, p. 193. [2] No italics in original.

as one which is entirely opposed to all positive evidence ; and can only further regard the superstructure he has built upon it (as to the function of the granular 'ids' and all their imaginary myriads of 'determinants,' composed in their turn of an endless variety of 'biophors') as based upon an extraordinary aggregate of assumptions and hypotheses, destined to support his particular views concerning heredity and evolution, but devoid of any independent support. His knowledge is vast, and his absolute sincerity is undoubted ; but his theories seem to have so thoroughly taken possession of him that he is unable to resist attempting to explain in detail things which it were wiser to regard as at present inexplicable.

Let us get back now a little more into the region of facts and of verifiable propositions, in regard to the all-important question as to the functions of the nucleus and its relation to hereditary phenomena. The interest here undoubtedly centres in the question of the function that is to be attributed to the chromatin granules (ids) which, when aggregated, go to form the chromosomes.

An important point not yet referred to is this. The chromatin is altogether differently arranged in different states of the cells. In what has been termed the 'resting stage,' but which, as Herbert Spencer says, would be better termed its 'growing stage,' the chromatin is irregularly dispersed throughout the nucleus in the form of a granular network ; and it is only when the time comes for division of the cell to occur that, as one of the first of the very complicated processes about to ensue, the chromatin granules begin to aggregate so as to form the particular number of band or rod-like 'chromosomes' peculiar to the cells of the plant or animal to which they belong.

Subsequently, as it has been undoubtedly proved, each chromosome, in the process of division, becomes split longitudinally into two equal portions ; the halves thus formed gradually separating from one another, and the whole of each set later on becoming aggregated so as to form one of the two daughter nuclei which now find themselves in the divided cytoplasm of the original cell. At this stage, the substance of the chromosomes again becomes distributed (by the separation of their component ids) so as to re-form the nuclear network of chromatin granules, characterising the cell during its growing stage. It is commonly believed that the chromatin of each chromosome preserves its identity during this time, so that when the next division of the cell occurs, the chromo-

somes are re-formed from exactly the same chromatin granules as before. These phenomena of karyokinesis are well shown in Fig. 74 of Weismann's "Evolution Theory," while the modification of the process that occurs in fertilised eggs is equally well shown in Fig. 75 of that work.

If attention is concentrated upon this latter process it may be easily understood that the fusion of the male nucleus with that of the female nucleus should have given rise to the inference that "these two nuclei convey respectively the paternal and maternal traits which are mingled in the offspring. And when there came to be discerned," as Herbert Spencer says,[1] "the karyokinesis by which the chromatin is, during cell-fission, exactly halved between the nuclei of the daughter-cells, the conclusion was drawn that the chromatin is more especially the agent of inheritance." There is, however, much cogency in what follows when he adds, "But though, taken by themselves, the phenomena of fertilisation seem to warrant this inference, the inference does not seem congruous with the phenomena of ordinary cell-multiplication—phenomena which have nothing to do with fertilisation and the transmission of hereditary characters. No explanation is yielded by the fact that ordinary cell-multiplication exhibits an elaborate process for exact halving of the chromatin. Why should this substance be so carefully portioned out among the cells of tissues which are not even remotely concerned with the propagation of the species? If it be said that the end achieved is the conveyance of paternal and maternal qualities in equal degree to every tissue; then the reply is that they do not seem to be conveyed in equal degrees. In the offspring there is not a uniform diffusion of the two sets of traits throughout all parts, but an irregular mixture of traits of the one with traits of the other."

H. Spencer then proceeds to make a suggestion which is in harmony with these facts, and well worthy of careful consideration. He calls attention to the chemical composition of chromatin, and its greater complexity compared with that of the general substance of the cell, seeing that analyses have shown it to contain an organic acid (nucleic) rich in phosphorus, combined with an albuminous substance: probably a combination of various proteids. This nuclear substance, therefore, has a special instability and tendency to undergo molecular change, such as nucleic acid has

[1] "Principles of Biology," 1898, Vol. I, p. 259.

been found to possess, and its presence in the cell may lead to the following results :—"Molecular agitation results from decomposition of each phosphorised molecule : shocks are continually propagated around. From the chromatin, units of which are thus ever falling into stabler states, there are ever being diffused waves of molecular motion, setting up molecular changes in the cytoplasm. The chromatin stands towards the other contents of the cell in the same relation that a nerve-element stands to any element of an organism which it excites." The distribution of the chromatin so as to form a diffuse network of granules during the growing stage of the cells is in a measure explained on this hypothesis, though from the point of view of the inheritance theory it would seem to be useless. This, among other facts harmonious with his suggestion is pointed out by Spencer when he says :—" During the interval between cell-fissions, when growth and the usual cell-activities are being carried on, the chromatin is dispersed throughout the nucleus into an irregular network ; thus greatly increasing the surface of contact between its substance and the substance in which it is embedded. As has been remarked, this wide distribution favours metabolism." So that, as he remarks, when contemplated from the suggested point of view karyokinesis as it occurs in the cells of almost all tissues is not wholly incomprehensible : "For if the chromatin yields the energy which initiates changes throughout the rest of the cell, we may see why there eventually arises a process for exact halving of the chromatin in a mother cell between two daughter cells." [1] The consequences of an unequal division in cells generally are then pointed out, and also the fact that the foregoing interpretation does not attempt to explain the special process known as karyokinesis : the argument implies merely that the tendency of Evolution is to establish *some* mode for the equal division of the chromatin.

An important fact is referred to by Verworn [2] which tends to support H. Spencer's view as to the stimulating functions of the nucleus. He calls attention to the existence in some cells of com-

[1] This view as to the functions of the nucleus seems capable of explaining the facts previously mentioned concerning sections of cells, and the regeneration only of those parts in which the nucleus was contained (p. 40) ; and also the results of Balbiani's experiments with Stentor, showing that the presence of only a sma l portion of the nucleus in a section of this organism was sufficient to admit of its complete regeneration (p. 41), leading le Dantec to the conclusion that it seemed to act like a chemical compound rather than as an organ.

[2] "General Physiology," Transln. 1899, p. 89.

plicated branched nuclei and then adds, quite irrespectively of any theory, "It seems noteworthy that it is the nuclei of secreting cells, *i.e.*, cells characterised by lively activity in which the principle of surface enlargement by branching is especially expressed."

There seems to be no definite way for reconciling this view as to the function of chromatin with the other view more commonly received, that it is the bearer of hereditary traits; though it may be said that this second function is not necessarily in conflict with the first. "While the unstable units of chromatin, ever undergoing changes, diffuse energy around, they may also be units which, *under the conditions furnished by fertilisation*, gravitate towards the organisation of the species. Possibly it may be that the complex combination of proteids, *common to chromatin and cytoplasm*, is the part in which the constitutional characters inhere; while the phosphorised component, falling from its unstable union and decomposing, evolves the energy which, ordinarily the cause of changes, now excites the more active changes following fertilisation."

We are here indeed introduced to a question which some of the most recent experiments on 'merotomy' have again brought forward: namely, whether experimental evidence does not show that hereditary traits may inhere in the cytoplasm as well as in the nucleus. Thus, in the third of an important short course of lectures recently delivered by Prof. Weldon at University College he is reported to have said,[1] "The nucleus has been held to be the sole vehicle of heredity by Weismann. Recent experiments of Boveri and Delage have demonstrated that this is incorrect. Both these writers have shown that denucleated eggs, produced by mechanical extrusion of nuclei, can be fertilised in the sea-urchin, and that they give rise to normal blastulæ, gastrulæ and larvæ. Hence the 'determinants' of heredity are present in the cell body of the egg. If such eggs are fertilised by alien spermatozoa (of different subspecies) the larva shows the characters of the male parent, because the determinants of these are more potent."

Exception was subsequently taken to this statement ("Lancet," Feb. 18th, p. 460), which seems to have been not quite sufficiently guarded, judging from the comment appended to the objection which was as follows:—"Boveri himself admits that the 'merogonie'[2]

[1] "The Lancet," Feb. 4, 1905, p. 308.

[2] A synonym for 'merotomy.'

experiments are equivocal because bastard larvæ, arising by the union of an entire egg with the spermatozoon of a distinct species, may also present the pure paternal form. At the same time, referring to the criticism raised by Pfeffer and others, that the spermatozoon is an entire cell and not merely a nucleus, he points out that this argument is not admissible in appraising the part played by the egg protoplasm in fertilisation. In bastard larvæ which have obtained nuclear substance only from the spermatozoon, in the fertilisation of egg fragments without nucleus ('merogonie'), the paternal type appears exclusively."

It certainly seems that in these experiments there is no positive evidence that the fragment of denucleated egg has communicated any of its female traits to the bastard offspring, although it has served as a matrix in, and with the aid of which (as mere nutritive material), the spermatozoon has been capable of giving rise to a larva of its own type. This must be regarded as a truly wonderful result, of a kind never before known: though it seems to be the converse of a class of facts hitherto well known, in which the eggs of certain animals are capable of going through all their phases of development altogether independently of contact and fusion with a spermatozoon.

This so-called 'parthenogenesis' or 'virgin reproduction' is now well known to be by no means a rare phenomenon. As Weismann says, "It occurs regularly and normally in many cases, especially in the very diverse groups of the great series of Arthropoda. Thus among insects it is found in certain saw-flies, gall-flies, ichneumon-flies, in the honey bee, and in common wasps, and it is particularly wide-spread among plant-lice (Aphides) such as the vine-aphis (*Phylloxera*), whose prodigious multiplication in a short time depends partly on the fact that all the generations with the exception of one, consist only of females with a parthogenetic mode of reproduction. Among the lower Crustaceans also parthenogenesis plays a large *rôle*, and in many species it even occurs as the sole mode of reproduction, but more often—as is also the case among insects—it occurs alternately with bi-sexual reproduction" (*loc. cit.* p. 303).

There is a very interesting point in connection with this occurrence of 'parthenogenesis,' and its relation to what occurs in ordinary bi-sexual reproduction. It is well known that the egg nucleus previous to fertilisation undergoes what are known as 'reducing divisions' (leading to the extrusion of the two

so-called 'polar bodies'), the effect of which is to leave the egg-nucleus with only half its usual number of chromosomes. A reducing division occurs also during the formation of spermatozoa, though at an earlier stage—the result being that the nucleus of the spermatozoon, as it enters the egg, also has only half the number of chromosomes peculiar to the species. The fusion of the two nuclei during the act of fœcundation restores to the egg its normal number of these bodies.

It becomes a matter of much interest to ascertain, therefore, what occurs when parthenogenesis takes place. Do the 'reducing' divisions occur as usual in the egg which develops without male influence, or does such an egg retain its full number of chromosomes? This interesting point has been settled by the researches of Weismann and others for some animals, and he is thus enabled to say that "those eggs of Aphides and Daphnids which are wholly parthenogenetic retain the full number of chromosomes of their species," owing to only one 'polar body' being extruded. Why, in different species, there should be different numbers of chromosomes; and why in a developing egg, in one way or another, it seems necessary that the full number of chromosomes should always be produced, still remain absolute enigmas; though a similar confession has to be made in regard to many of the phenomena of karyokinesis generally.

In this connection other facts of extreme interest must be mentioned. In some silk-moths and hawk-moths, and in quite a number of other Lepidoptera, it sometimes happens that out of a large number of unfertilised eggs a few will develop and produce caterpillars. But, as Weismann says, these facts gain "increased importance through the investigations of the Russian naturalist, Tichomiroff, who succeeded in considerably increasing the number of unfertilised eggs that developed by gently rubbing them with a paint-brush, or by dipping them for a little in dilute sulphuric acid. It is thus possible to make eggs which would not ordinarily develop without being fertilised, capable of parthenogenetic development by means of a chemical stimulus. This sounds almost incredible, but it is beyond a doubt, and it is still further corroborated by the fact that Prof. Jacques Loeb has succeeded in inciting the eggs of a sea-urchin to parthogenetic development by means of a chemical stimulus. When he added to the sea water in which the eggs were laid a certain quantity of chloride of magnesium the ova developed, and not only went

through the process of segmentation, but even reached the stage of the quaint easel-like Pluteus larva.[1]

In both these latter sets of experiments it is to be presumed that the eggs had undergone the usual reducing division and that the number of chromosomes had been reduced to half their usual number: it is extremely interesting to find, therefore, that physical or certain common chemical stimuli were in these cases capable of replacing the stimulation usually induced by (the chemical and molecular) changes emanating from spermatozoa. Then, again, there can be no doubt that in the 'merogonie' experiments in which de-nucleated eggs of the sea-urchin were fertilised by the spermatozoon of another sub-species, we have a related though opposite form of 'unisexual' development taking place, notwithstanding the presence of only one half (in the spermatozoan nucleus) of the ordinary number of chromosomes belonging to the species. This latter case is, however, far more remarkable than the other variety of 'unisexual' development, because of the minute size of the spermatozoon as compared with that of the ovum.

Facts very similar to those we have just been recording are known to occur also in the vegetable kingdom, and especially among ferns, as was brought out by a skilled writer in "The Times" (January, 1904) when calling attention to a remarkable discovery laid before the Royal Society by Prof. Farmer, in conjunction with Messrs. Moore and Walker, in regard to the chromosomes in many of the cells of cancerous growths. From this article I venture to make the following interesting quotation concerning these new investigations :—

"They have grown out of a close study of the reproductive processes in animals and plants, and furnish an example of the practical results which frequently follow from what we are too often apt to disregard as the pursuit of pure science."

"Your readers may be familiar with the fact that a fern forms spores, these same spores when ripe fall upon the ground, and there grow into little heart-shaped plants which are not in the least like ferns, and are called by botanists prothallia. It is upon

[1] According to Weldon ("The Lancet," February 4th) Loeb has since succeeded in bringing about similar results "by the presence of a large quantity of carbon dioxide in the sea water"—experiments which have been confirmed by Delage. The latter investigator has quite recently recorded other remarkable results ("Compt. Rend.," May 22, 1905, p. 1369) with a solution of chloride of manganese in distilled water, and also with a solution of monobasic phosphate of soda.

these new plants that the sexual cells are produced, which after fusion grow up into a fern. Thus the life of a fern is cyclical, and one of the most remarkable results of recent biological inquiry has been the demonstration of the fact that the cells of the fern differ from those of the prothallium in that they contain just twice as many little rod-like bodies which stain with aniline dyes and have been called chromosomes. In the fern and the prothallium these numbers remain constant, and the reduced number in the prothallial cell is brought back again to that of the original fern only by the fusion of the sexual elements."

"Now it has been found that in some ferns spores need not be formed, but that the reduced prothallial tissue sprouts out upon the fronds in little masses, while in others the same result can be produced by artificial conditions, by pinning the fronds down on damp earth, whereupon after a time the prothallial growth spreads like a cancerous development round the edges of the frond."

"It has long been known that the cells of animals, like those of plants, contain constant numbers of chromatic rods, and it is further known that a similar reduction in these numbers takes place regularly in the development of the sexual cells; but what has not hitherto been suspected is the fact first pointed out in the paper already referred to, that the changes witnessed in the development of cancerous tissue are identical with those occurring in the normal sexual reduction of the tissues both of animals and plants."

"From the results of these observations, if correct, we reach, in fact, the singular conclusion that a cancerous growth in man is precisely similar to the abnormal production of prothallial tissue on the fronds of a fern; or, in other words, that the onset of this terrible human scourge is indirectly due to some of those not as yet fully understood causes which, like the tying down of the frond of a fern, induce the normal tissues of the body to enter on the reproductive or 'reduced' phase of its life cycle."

One of the authors of this investigation in a later communication, in July last, to the Pathological Society[1] recorded another interesting fact closely related to the observations of the Russian naturalist and those of Prof. Loeb. Thus C. E. Walker says in reference to the observations on cancer cells, "The occurrence

[1] See 'Transactions' of the Society, Vol. LV, p. 454.

of reduced cells naturally suggests the probability or possibility of fusion, and to us it did so particularly in view of what happened in the case of certain ferns observed by Prof. Farmer, Mr. Moore, and Miss Digby more than a year ago. Here the application of suitable stimuli to prothallial tissue caused the nuclei of adjacent cells to fuse, fertilisation and the production of the succeeding generation taking place in some cases without any sexual elements appearing."

This change in the cells of cancer, representing an incipient conversion of them into the forms of germ-cells, must be regarded as decidedly adverse to the views of Weismann as to the absolute distinction between the 'somatic-cells' and the germ-cells and the 'continuity' of the latter. His views moreover seem negatived by multitudes of well-established facts concerning budding, 'regeneration' and repair of injuries generally, to which we must now turn our attention.

Our knowledge concerning these processes in various animals has been greatly increased during recent years, though the power of propagating themselves by buds and cuttings possessed by all kinds of plants has long been thoroughly familiar. In some Algæ, such as Caulerpa, in Liverworts, and in Bryophyllum, one of the House-leeks, this power is possessed to an extreme degree, so that even minute fragments of such plants are capable of growing and developing into perfect plants. While, as we are told by Dr. (now Sir Wm.) Hooker,[1] there is "a species of Begonia the stalks, leaves and other parts of which are superficially studded with loosely attached cellular bodies," and "any one of these bodies, if placed under favourable conditions, will produce a perfect plant similar to its parent."

In animals this power of repair is by no means so wide-spread—which is only what might be anticipated having regard to the increasing complexity of organisation met with in the higher types, and to the fact that they are not mere repetitions of more or less similar parts, such as obtains so largely among plants. Among lower animal organisms we find the property wide-spread and very remarkable. Beginning with 'unicellular organisms'[2]; it is seen also among different kinds of Polyps, Medusæ, Starfishes,

[1] Report of Brit. Association, 1868.

[2] See the Experiments of Balbiani with Stentor (pp. 40, 41) and the results of the section of cells generally.

Planariæ and other worms (though not among Free Nematoids); till, when we come to Crustacea, Insects, and Amphibia the power of reproduction after injury dwindles down, and is confined in the main to a power of regenerating lost limbs or appendages, and in Mammalia to a power of repairing wounds.

In some of these lower organisms the power of repair is just as great as it is among some plants, as Weismann himself fully admits. Thus concerning one of the common Polyps he says:—"Not only has Hydra been cut in from two to twenty different pieces, but it has even been chopped up into innumerable fragments, and yet each of these, under favourable circumstances, was able to grow again into a complete animal." Then, concerning certain common worms, Planariæ, he says:—"Through the experiments of Loeb, Morgan, Voigt, Bickford, and others, we know that these animals respond to almost every mutilation by complete reconstruction, that they may, for instance, as is indicated in Fig. 96, be cut transversely into nine or ten pieces, with the result that each of these pieces grows again to a whole animal, unless external influences are unfavourable and prevent it." While in regard to another little worm found in ponds, Lumbriculus, he adds, "Certainly the power of regeneration is so great in this animal that it is out of the question to talk of localising the primary constituents of regeneration; almost every broken surface is capable of regeneration."

In his two lectures on "Regeneration" Weismann makes an ingenious but laboured attempt to bring all this class of facts into harmony with his views as to the 'Continuity of the Germ Plasm,' and the all-powerful influence, even in lowest organisms, of 'natural selection.' Thus he says:—"My own view is that the regenerative capacity is not something primary, but rather an adaptation to the organism's susceptibility to injury, that is, a power which occurs in organisms in varying degrees, proportionate to the degree and frequency of their liability to injury. Regeneration prevents the injured animal from perishing, or from living on in a mutilated state, and in this lies an advantage for the maintenance of the species." But even in accordance with this view that the regenerative capacity is a secondary and acquired, rather than a primary, property of the living matter of the organisms possessing it, he is obliged to postulate the existence in all kinds of cells, in the animals in question, of "inactive accessory idioplasm,"—that is, of hosts of his determinants, usually latent, but capable of

being roused into activity, as this or that accident may require; while as regards plants he is compelled to assume that "cells must be distributed over the whole surface of the plant, each of which can in certain circumstances become the starting point of a bud. That is to say, each must contain, *in a latent state the complete germ-plasm which is necessary for the production of an entire plant.*" The way in which all these vast assortments of determinants would need to be distributed for possible use, in addition to all those which are required by Weismann's theories for actual use, afford problems quite bewildering in their complexity.

Let us turn now to an opposite and much simpler mode of accounting for all these facts concerning budding, regeneration and repair which was published by Herbert Spencer in his "Principles of Biology" so long ago as 1863; and which, twelve years ago, was still maintained by him in a celebrated controversy with Weismann himself in the pages of "The Contemporary Review" for 1893-4.

After referring to some of the facts concerning 'regeneration' known at the time, H. Spencer says:—"We must infer that a plant or animal of any species is made up of special units, in all of which there dwells the intrinsic aptitude to aggregate into the form of that species: just as in the atoms of a salt there dwells the intrinsic aptitude to crystallise in a particular way. It seems difficult to conceive that this can be so; but we see that it *is* so. Groups of units taken from an organism (providing they are of a certain bulk and not much differentiated into special structures) *have* this power of rearranging themselves; and we are thus compelled to recognise the tendency to assume the specific form as inherent in all parts of the organism. Manifestly too, if we are thus to interpret the reproduction of the organism from one of its amorphous fragments, we must thus interpret the reproduction of any minor portion of an organism by the remainder . . . In the one case as in the other, the vitalised molecules composing the tissues show their proclivity towards a particular arrangement; and whether such proclivity is exhibited in reproducing the entire form, or in completing it when rendered imperfect, matters not."

He then goes on to say:—"For this property there is no fit term. If we accept the word polarity as a name for the force by which inorganic units are aggregated into a form peculiar to them, we may apply this name to the analogous force displayed by

organic units. But, as above admitted, polarity as ascribed to atoms is but a name for something of which we are ignorant—a name for a hypothetical property which as much needs explanation as that which it is used to explain. Nevertheless, in default of another word, we must employ this; taking care, however, to restrict its meaning. If we simply substitute the term polarity for the circuitous expression, the power which certain units have of arranging themselves into a special form, we may, without assuming anything more than is proved, use the term organic polarity or polarity of the organic units to signify the proximate cause of the ability which organisms display of reproducing lost parts, or of their having assumed the shape and structure which is peculiar to them."

Enquiring next into the possible nature of these units "which possess the property of arranging themselves into the special structure of the organisms to which they belong" he comes to the conclusion that they cannot be any of the complex chemical compounds entering into the composition of living matter. On such a supposition the existence of endlessly varied forms of animals and plants would be inexplicable; so that he dismisses the notion of their being 'chemical units.' He similarly dismisses the notion that the powers in question can reside in 'morphological units' or cells, as, independently of other reasons, "the formation of a cell is to some extent a manifestation of this same peculiar power." [1]

He then goes on to say:—"If then this organic polarity can be possessed neither by the chemical units nor the morphological units, we must conceive it as possessed by certain intermediate units, which we may term *physiological*. There seems no alternative but to suppose that the chemical units combine into units immensely more complex than themselves, complex as they are; and that in each organism the physiological units produced by this further compounding of highly compound atoms, have a more or less distinctive character. We must conclude that in each case some slight difference of composition in these units leading to some slight difference in their mutual play of forces, produces a difference in the form which the aggregate of them assumes."

These 'physiological units' of Herbert Spencer would, therefore, correspond almost exactly with the elementary vital units of

[1] *Loc. cit.* Vol. I, § 65. The old numbering of the sections is preserved in the new edition of 1898.

Haeckel (plastidules) and of Verworn (biogens)—the existence of which was postulated by them at later periods.

After a careful consideration of all the facts known at the time, what may be termed a 'Law of Heredity' was formulated by H. Spencer, in which he says[1]:—"Bringing the question to its ultimate and simplest form, we may say that, as on the one hand physiological units will, because of their special polarities, build themselves into an organism of a special structure; so, on the other hand, if the structure of this organism is modified by modified function, it will impress some corresponding modification on the structure and polarities of its units. The units and the aggregate must act and react on each other. The forces exercised by each unit on the aggregate and by the aggregate on each unit must ever tend towards a balance. If nothing prevents, the units will mould the aggregate into a form in equilibrium with their pre-existing polarities. If, contrariwise, the aggregate is made by incident actions to take a new form, its forces must tend to remould the units into harmony with this new form. And to say that the physiological units are in any degree so remoulded as to bring their polar forces towards equilibrium with the forces of the modified aggregate, is to say that when separated in the shape of reproductive centres, the units will tend to build themselves up into an aggregate modified in the same direction.

Among simple organisms, as we have seen, almost any part of the substance which separates, or is separated, from one of them is capable of developing into a similar simple organism. But as organisms grow more and more complex in their structure, so we find that a difference arises in the reproductive powers of different tissues—till at last the capacity to reproduce the entire organism (either without fertilisation or only after this has occurred) becomes in animals restricted to the morphological units which are produced in special organs.[2] How much this restriction of the reproductive function is due to a general specialisation is obvious from the fact that it is most marked where complexity of organisation attains its maximum. Complexity of structure necessarily carries with it

[1] "Principles of Biology," vol. i. § 84.

[2] The necessity for the fertilisation of some of these reproductive elements, and the evolution of sexual differences among the animals and plants in which this necessity obtains, is merely a superadded complexity—a difference of degree and not of kind. The fundamental phenomena of reproduction are essentially similar in sexual and sexless organisms. (See Spencer's "Principles of Biology," vol. i. § 77.

complexity of function, and in proportion as distinct functions are performed by special parts of the organism, so are the several parts more and more bound together into one organic whole.

This difference is well seen between plants and highly-organised animals. The plant, it is true, develops seeds and pollen in special parts or organs; but just as the plant, taken as a whole, is to a great extent a repetition of similar parts whose organisation is by no means so complex, so do these separate parts, when severed from the parent organism, retain that generative power which enables them, under suitable conditions, to grow into plants of a similar kind. But in animals even comparatively low in the scale of complexity all this is changed. They are not mere repetitions of similar parts—each having a potential individuality of its own: they are rather aggregations of different parts bound together and constituting one organic whole by means of vascular and nervous systems which serve as bonds of unity. The higher the grade of development of the organism—the more its tissues have become differentiated—the less are they severally endowed with a reproductive power, even of a partial kind. In such organisms, we find that each part has a distinct function to perform, and therefore the reproductive function is restricted to the elements produced in definite organs. Although restricted in their place of origin, however, there is reason to believe that sperm-cells and germ-cells are comparatively unspecialised products. After a careful summary of what is known on the subject, H. Spencer says:—"The assumption to which we seem driven by the *ensemble* of the evidence is, that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to. . . . Thus, the phenomena of Heredity are seen to assimilate with other phenomena; and the assumption which these phenomena thrust on us appears to be equally thrust on us by the phenomena of Heredity. We must conclude that the likeness of any organism to either parent is conveyed by the special tendencies of the physiological units derived from that parent. In the fertilised germ we have two groups of physiological units, slightly different in their structures. These slightly different units severally multiply at the expense of the nutriment supplied to the unfolding germ, each kind moulding the nutriment into its own type. Throughout the process of evolution the two kinds of units mainly agreeing in their polarities

and in the form which they tend to build themselves into, but having minor differences, work in unison to produce an organism of the species from which they are derived, but work in antagonism to produce copies of their respective parent organisms; and hence ultimately results an organism in which traits of the one are mixed with the traits of the other." [1]

Professor Weldon, in his recently-delivered lectures on "Current Theories of the Hereditary Process," also comes to the conclusion that Weismann's theories concerning the 'sorting of determinants' are "quite irreconcilable with the facts of the equipotential nature of the blastomeres in the early stages of cell-division [of the ovum] and with the facts of regeneration of lost parts." He adds:— "The changes which subsequently occur in the different cell lineages derived from the fertilised germ-cells are largely due to other factors—viz., the relative positions assumed with regard to each other and to the cell mass by individual cells, and the differences of the incidental stimuli of the environment on differently placed cells (gravity, light and shade, pressure, relative food supply) . . . Hence for normal development and perfect embyrogenesis a very constant and 'correct' environment is necessary."

Weismann indeed admits that if the power of repair and regeneration is dependent upon a primary property rather than upon one that has been acquired through 'natural selection' he would have to give up his position, since he says (Vol. II, p. 19), "in truth if the body were really able to replace, after artificial injury, parts which are never liable to injury in natural conditions, and to do so in a most beautiful and appropriate manner, then there would be nothing for it but at least to regard the faculty of regeneration as a primary power of living creatures, and to think of the organism as like a crystal, which invariably completes itself if it be damaged in any part."

[1] The degrees and modes in which the several ancestral traits are mixed, are now being keenly investigated and discussed by two rival schools—one of which favours the views of Francis Galton, and the other the newly-discovered Mendelian hypothesis, which is advocated by Hugo de Vries and Bateson. The views of Galton are favoured by Weldon and Karl Pearson. He believes that the two parents contribute between them $\frac{1}{2}$ of the total heritage; the four grand-parents $\frac{1}{4}$; and the eight great grand-parents $\frac{1}{8}$ of the total heritage, and so on. The differences between the two views seem to depend greatly upon whether the chromosomes always divide "transversely" or "horizontally" (See Weldon "Lancet," Mar. 25, p. 810).

CHAPTER VI

SOME FACTORS OF EVOLUTION: ORGANIC POLARITY AND MUTATION

WE have seen how great are the tendencies to molecular changes in all kinds of bodies, and how from its very nature and the great complexity of the molecules entering into its composition these tendencies ought to attain their maximum in living matter—that is in protoplasm, the physical basis of life. We have seen how in simpler forms of matter of the crystalloid type, changes in molecular composition are often comparatively easily brought about under the influence of changing conditions, with the result that new crystalline forms as well as other new physical properties are assumed by the matter in question as a consequence of the altered polarities of their molecules. The same kind of thing should hold good, therefore, for the different varieties of living matter. This is frankly admitted by Herbert Spencer, who says[1] :—"These truths are not limited to inorganic matter: they unquestionably hold of organic matter. As certainly as molecules of alum have a form of equilibrium, the octahedron, into which they fall when the temperature of their solvent allows them to aggregate, so certainly must organic molecules of each kind, no matter how complex, have a form of equilibrium in which, when they aggregate, their complex forces are balanced—a form far less rigid and definite, for the reason that they have far less definite polarities, are far more unstable, and have their tendencies more easily modified by environing conditions."

This statement, and many others of a similar nature in the work of Herbert Spencer, is in marked contrast with what is to be met in the last and most elaborate work of Prof. Weismann, "The Evolution Theory," which I have very carefully studied. On only one single page (Vol. II, p. 152) have I found any mention of the word 'isomerism'; while there have been no references to the possibility of variations in forms of organisms being due to varia-

[1] "Principles of Biology," Vol. I, Append. D.

tions in the molecular composition of living matter; the author dwells rather upon 'nutritive' variations affecting his 'determinants' in the germ-plasm. Indeed, he even goes so far as to say (Vol. II, p. 204), "In my opinion, at all events, there is no such thing as a 'tendency' of the protoplasm to vary."

It so happens, moreover, that none of the prominent writers on the evolution theory seem to have devoted much consideration to the question of the variability of the lower forms of life; and what is still more surprising is the fact that none of them seem to appreciate how incompatible is the existence of myriads of the lowest forms of life at the present day with the commonly accepted belief that all the forms of life which have ever lived upon the earth have gradually developed, through endless generations, from primordial forms first appearing upon its surface when it became cool enough for the existence of living things. All this astounding evolution has, as they assume, taken place by virtue of the plasticity of living matter in co-operation with other varying factors; and yet the surface of the earth is found to swarm everywhere at the present day with infinite multitudes of organisms of the lowest type. How utterly contradictory this seems to be.

Yet notwithstanding this remarkable discrepancy between fact and theory, biologists make no attempt to solve the difficulty. At the same time they either deny or try to explain away the cogency of the evidence pointing to the occurrence of Archebiosis and Heterogenesis—though such occurrences seem alone capable of reconciling facts and theories. Certainly, on the assumption of only one natural origin of living matter in a remote geological past no satisfactory account can be given of the present existence of swarms of lowest, highly variable, living organisms which would be at all in accord with the doctrine of Evolution. Some, it is true, grant that a natural origin of living matter may have occurred many times in the past, though they believe there is no evidence of its occurrence, or of the occurrence of Heterogenesis at the present day: consequently they remain equally unable to give any rational account of the present existence almost everywhere of countless swarms of the lowest types of life.

Even Herbert Spencer, for once, seems to have yielded to the voice of authority, emanating from his friend Huxley, and to have been persuaded that no positive evidence could be, or had been, obtained by means of experiment demonstrating the *de novo* origin of living matter in the present day. And no good evidence of

Heterogenesis being forthcoming, at the time that he wrote, he remained also unconvinced as to the reality of its occurrence. As a result, he never attempted to grapple with this striking contradiction between facts and theory to which I have just been referring.

On the supposition that all existing forms of life were, in some way, descendants of forms which had, as he fully believed, come into being *de novo*, when the surface of the earth had cooled sufficiently to permit of the material combinations needful for their origin, he must have seen himself confronted by a great difficulty. Under these circumstances he never even ventured to apply his doctrine of 'organic polarity' so as to attempt to account thereby for the forms assumed by the lowest types of life. Thus, speaking of this supposed new-born living matter in the remote past, and assuming that it would have appeared as portions of protoplasm more minute and more indefinite than the lowest known form of Amœbæ, he says:—"The evolution of specific shapes must, like all other organic evolution, have resulted from the actions and reactions between such incipient types and their environments, and the continued survival of those which happened to have specialities best fitted to the specialities of their environment. To reach by this process the comparatively well-specialised forms of ordinary Infusoria must, I conceive, have taken an enormous period of time." He is even more positive in rejecting other phenomena which, as I shall hope to show in later chapters, occur with extreme frequency. Thus he says:—"Not only the established truths of Biology, but the established truths of science in general, negative the supposition that organisms having structures definite enough to identify them as belonging to known genera and species, can be produced *in the absence of germs derived from antecedent organisms of the same species*." [1]

Later on I shall claim to show that this, which was looked upon as so utterly improbable by Herbert Spencer, is nevertheless constantly occurring all around us; and that the products of such processes are continually appearing and giving rise to the myriads of those lowest forms of life, whose present existence would otherwise be so impossible for the Evolutionist consistently to explain.

Nay more, I shall claim that such processes can only receive their explanation by reference to the innate tendency of living

[1] *Loc. cit.* Vol. I Append. D. (p. 698). This was written about the year 1866: some years before I had attempted to deal with these questions.

matter to undergo isomeric changes (such as Spencer freely admits and Weismann denies), coupled with Spencer's doctrines of 'organic polarity' and 'physiological units.'

The sentence quoted from Herbert Spencer near the commencement of this chapter shows plainly enough what he wished us to understand by 'organic polarity.' If the molecules of alum during the process of crystallisation had, under altered conditions, undergone some isomeric change, they would no longer have been able to group themselves into the old form of the octahedron; a new mode of equilibrium among their molecules would have resulted in a new crystalline form and other new properties, as in other cases previously quoted (pp. 44–47). But, as applied to lowest forms of living matter, the doctrine of 'physiological units' and of 'organic polarity' means surely that the forms of low organisms may be direct results of the attempts to establish a moving equilibrium of the particular units entering into their composition. As with the crystals, let us suppose that, under the influence of some new conditions, isomeric changes have been induced in the molecular constitution of the pre-existing physiological units of some simple organism, this should in accordance with Spencer's doctrine result in the appearance of a new organic form in some brief period and not, as he strangely enough suggests, in the passage previously quoted, gradually and during a succession of generations, extending over "an enormous period of time."

This latter cannot be the legitimate interpretation of his doctrine, as he himself shows in a later page (p. 705) when he says in reference to his hypothesis of physiological units:—"In its complete form then the conception is that these specific molecules, having the immense complexity above described, and having correspondently complex polarities which cannot be mutually balanced by any simple form of aggregation, have, for the form of aggregation in which all their forces are equilibriated, the structure of the adult organism to which they belong; and that they are compelled to fall into this structure by the co-operation of the environing forces acting on them, and the forces they exercise on one another—the environing forces being the source of the *power* which effects the re-arrangement, and the polarities of the molecules determining the *direction* into which that power is turned."

Here then is one of the Factors of Evolution, of which no account is taken by Prof. Weismann, but which my observations on Heterogenesis compel me to regard as *the* all-powerful cause of

Evolution among the lowest kinds of living things. How far up in the scale of life it remains an all-powerful cause must for the present remain unknown. Certainly, my observations compel me to believe that organisms as high as Ciliated Infusoria very frequently derive their form as a direct outcome of molecular changes in masses of living matter derived from wholly different sources, as I shall show later on. This 'organic polarity' is probably all-powerful also among all kinds of Protophyta and Protozoa—in many of which the wonderful symmetry of their forms and markings is strongly suggestive of the determining influence of polarities, and the production of a kind of organic crystallisation. Among Protophyta look, for instance, at the forms of Desmids; at the forms and surface patterns of Diatoms; and among other Algæ at the extremely symmetrical patterns or modes of grouping of the cells in such forms as Coleochœte, Pediastrum and Volvox. While among the Protozoa evidence of the same kind of forces having been at work is to be found in the single chambered shells of the Arcellinæ, and still more plainly in the beautiful forms of Foraminifera, and of the Radiolaria. Speaking of the latter creatures Haeckel says,[1] "All the various fundamental forms, that can be distinguished and defined mathematically, are found realised in the graceful flinty skeletons of these unicellular sea-dwelling protozoa. I have distinguished more than 4,000 forms of them, illustrated by 140 plates, in my monograph on the Challenger-radiolaria."

No reasonable person can suppose that any distinct advantage could accrue to the possessors from these infinitely varied shapes and surface patterns. And if so, the cause of their origin must be something altogether outside the pale of natural selection, upon which Weismann doubtless would wish us to rely here, as in all other respects.

But this same cause which may be all-powerful in determining the actual shapes and superficial markings of the lowest forms of life, is one that cannot fail to be operative as a contributing morphological cause in altogether higher forms of life, and thus in part to account for the symmetry displayed in the forms of plants and animals generally—and especially for the radial or bilateral symmetry of the latter. For as Bateson points out,[2] symmetry is "a character whose presence among organisms

[1] "The Wonders of Life," 1904, p. 178.

[2] "Materials for the Study of Variation," 1894, p. 22.

approaches to universality." The existence of patterns, he adds, "again and again recur, and again and again the question of their significance goes unanswered." It is not only in regard to "large and tangible structures that the question arises, for the same challenge is presented in the most minute and seemingly trifling details." But may not a good clue as to the meaning of all this be fairly said to exist in the abiding influence of 'organic polarity'?

In considering the possible influence of 'physiological units' and 'organic polarity' in determining the forms of living things we must not restrict our consideration to simple cases in which the egg-substance is sufficient in mass to enable the physiological units, without the aid of additional material, to undergo equilibration, and thus to permit the specific forms of the organisms being assumed within the egg-envelopes—as happens, for instance, in the development of Rotifers and Nematodes.

Much more complicated cases have also to be considered, which are thus referred to by Herbert Spencer (*loc. cit.*, p. 706):—"But among higher animals such direct transformations cannot happen. The mass of physiological units required to produce the size as well as the structure that approximately equilibrates them is not all present, but has to be formed by successive additions—additions which in viviparous animals are made by absorbing, and transforming into these special molecules, the organisable materials directly supplied by the parent, and which in oviparous animals are made by doing the like with the organisable materials in the 'food-yelk,' deposited by the parent in the same envelope with the germ. Hence it results that, under such conditions, the physiological units which first aggregate into the rudiment of the future organism do not form a structure like that of the adult organism, which, when of such small dimensions, does not equilibrate them. They distribute themselves so as partly to satisfy the chief among their complex polarities. The vaguely-differentiated mass thus produced cannot, however, be in equilibrium. Each increment of physiological units formed and integrated by it changes the distribution of forces; and this has a double effect. It tends to modify the differentiations already made, bringing them a step nearer to the equilibrating structure; and the physiological units next integrated, being brought under the aggregate of polar forces exercised by the whole mass, which

now approaches a step nearer to that ultimate distribution of polar forces which exists in the adult organism, are coerced more directly into the typical structure. Thus there is necessitated a series of compromises. Each successive form assumed is unstable and transitional: approach to the typical structure going on hand in hand with approach to the typical bulk."

This may seem very complicated; but it appears to be a condition inseparable from all attempts to explain organisation and heredity. What has to be steadily borne in mind is the fact that this hypothesis is quite in accord with all the known data as to regeneration and repair in animals; as well as with the phenomena of reproduction by buds, cuttings, and superficial scales in plants, to which reference has previously been made (pp. 89–91); while Weismann's counter hypothesis concerning 'ids' and 'determinants' utterly fails to give any rational account of such phenomena.

And now it seems clear that what Spencer says in this last quotation may help us to understand the possibility of the occurrence from time to time of what are known as 'sports,' arising during the development of plants and animals—such as have been long known to occur and have been habitually taken advantage of by breeders in their efforts to establish new races or new varieties. The occurrence of such sports has in recent years been much dwelt upon by Hugo de Vries[1] in the foundation of his doctrine of the origin of species by 'Mutation,' as also by W. Bateson in his work on 'Discontinuous Variation.'

Just as in a gemma or egg of one of the lower organisms when subjected to unnatural conditions, isomeric changes in the living substance may be brought about, leading to the production of an entirely different form of life (Heterogenesis), so, it seems to me, we must look to some isomeric changes occurring in particular parts of the living substance of a plant or animal, during some of its later developmental phases, in order to account for the occurrence of 'sports' and perhaps for the origin of new species by 'Mutation,' after the manner made known by de Vries.

The investigations of this latter distinguished worker have, indeed, cast quite a new light upon the importance of this subject and, together with the researches of Neo-Lamarckians, seem to necessitate a readjustment of our notions as to the range of action and as to the *modus operandi* of 'natural selection.'

[1] "Die Mutationstheorie," Leipzig, 1901.

It is well known that Darwin did not attach any very great importance to the existence of 'sports,' and that his doctrine of 'natural selection' relied in the main on the constant occurrence of minute individual variations in all directions in the different representatives of species, some of which would be more and some less 'useful' for their possessors in their struggle for existence: leading in this way through repeated processes of multiplication and descent to the gradual extinction of those possessing the least useful qualities, and, on the other hand, as Herbert Spencer termed it, to "survival of the fittest."

Some very fundamental objections have for a long time been raised against the adequacy of these particular views to account for the origin of new species, and such objections have been gathering weight during recent years. Thus, it was contended, even as far back as 1871 by St. George Mivart,[1] that, among the minute individual variations which arise, many would have no utility for the species; and that the incipient stages of a possibly useful variation could not be seized upon and developed by a process of 'natural selection,' so that these incipient variations would be liable to be swamped by crossing or to disappear by atavism.

The American investigators, Packard, Cope, and Hyatt also dwell upon the inadequacy of Natural Selection as a cause of Variation. According to Packard it comes in as a cause of the preservation or extinction of forms that have arisen in other ways.[2] And as Cope says, "A selection cannot be the cause of those alternatives from which it selects. The alternatives must be presented before the selection can commence."[3]

Then again, looking at the question from another point of view, it has been said by J. T. Cunningham[4] that the theory of natural selection is "only a theory of the origin of adaptations," while it is contended that "there is scarcely a single instance in which a specific character has been shown to be useful, to be adaptive." Bateson dwells upon a similar point of view when he says,[5] "as Darwin and many others have often

[1] "Genesis of Species," Chap. II.

[2] "Life of Lamarck," 1901, p. 391. This book is a storehouse of facts and references bearing on Neo-Lamarckism.

[3] "The Primary Factors of Organic Evolution," 1896, p. 474.

[4] Quoted by H. Spencer, *loc. cit.*, p. 565; but see his work on "Sexual Dimorphism in the Animal Kingdom," 1900, pp. 8–11.

[5] "Material for the Study of Variation," 1894, p. 11.

pointed out, the characters which visibly differentiate species are not as a rule capital facts in the constitution of vital organs, but more often they are just those features which seem to us useless and trivial. . . . These differences are often complex and are strikingly constant, but their utility is in almost every case problematical."

Lastly, W. Bateson (*loc. cit.*, p. 5) refers to another difficulty which is common for the theory of Lamarck as well as for that of Darwin. He says:—"In the way of both solutions there is one cardinal difficulty which in its most general form may be thus expressed. According to both theories specific diversity of form is consequent upon diversity of environment, and diversity of environment is thus the ultimate measure of diversity of specific form. Here then we meet the difficulty that diverse environments often shade into each other insensibly and form a continuous series, whereas the Specific Forms of life which are subject to them on the whole form a Discontinuous Series . . . forms which are apparently identical live under conditions which are apparently very different; while species which though closely allied are constantly distinct are found under conditions which are apparently the same." On a subsequent page (p. 17) the same writer says, "The first question which the Study of Variation may be expected to answer, relates to the origin of that Discontinuity of which Species is the objective expression. Such Discontinuity is not in the environment; may it not, then, be in the living matter itself?"

Fundamental difficulties of this kind de Vries claims to meet and answer by his 'Mutation theory,' the details of which he has recently published after many years of laborious but most fruitful research. Some of the old examples may, however, first be cited of this more or less sudden cropping up of 'sports' or actual new species, before dwelling further on the views put forward by de Vries.

One of the most remarkable of known instances of *per saltum* development is the fact recorded by Darwin[1] that on five separate occasions what Sclater has pronounced to be a distinct species of Peacock—the "black shouldered kind," or *Pavo nigripennis*—has appeared suddenly in a stock of common or pied peacocks, and in two of the cases (that is, in the flocks of Sir J. Trevelyan and

[1] "Animals and Plants under Domestication," vol. i., p. 322.

Mr. Thornton) the black shouldered kind had increased "to the extinction of the previously existing breed."

Then, again, as J. J. Murphy says[1]:—"The otter, or Ancon, sheep of North America was also the result of a sudden variation; and the differences in the form of its skeleton from that of the common sheep amounted to a specific if not a generic difference." It is true, as this writer says, these and other instances that could be mentioned have arisen under domestication.[2] But he pertinently asks why may not the same kind of thing be taking place in the wild state? "It may be true that we have no evidence of the origin of wild species in this way. But this is not a case in which negative evidence proves anything. We have never witnessed the origin of a wild species by any process whatever; and if a species were to come suddenly into being in the wild state, as the Ancon sheep did under domestication, how could we ascertain the fact? If the first of a newly-begotten species were found, the fact of its discovery would tell nothing about its origin. Naturalists would register it as a very rare species, having been only once met with, *but they would have no means of knowing whether it were the first or last of its race.*"

The instances just cited are cases in which new species have suddenly appeared without known cause—no one has been able to fix upon any external determining conditions. Other instances, however, are known where, as with the famous metamorphosis of the Axolotl, a more or less definite and known change of conditions has helped to bring about the transformations in question.

One such case, where a marked alteration in climate was operative, has been cited by Darwin.[3] Metzger obtained seeds of a tall kind of maize (*Zea altissima*) from the warm regions of America and cultivated them in Germany. During the first and second years the plants reared showed some differences from the parent stock, and, "In the third generation nearly all resemblance to the original and very distinct American parent-form was lost. In the sixth generation this maize perfectly resembled a European variety. . . . When Metzger published his book, this variety was

[1] "Habit and Intelligence," 1869, vol. i., p. 343.

[2] "It is certain that the Ancon and Mauchamp breeds of sheep, and almost certain that the Niata cattle, turnspit and pug-dogs, jumper and frizzled fowls, short-faced tumbler pigeons, hook-billed ducks, &c., and with plants a multitude of varieties, suddenly appeared in nearly the same state as we now see them" (Darwin, *loc. cit.*, vol. ii., p. 414).

[3] "Animals and Plants under Domestication," vol. i., p. 322.

still cultivated near Heidelberg, and could be distinguished from the common kind only by a somewhat more vigorous growth." Darwin speaks of this as the most remarkable instance known to him "of the direct and prompt action of climate upon a plant."

Then, again, a very remarkable series of sudden transformations in the character of the flower in many different kinds of plants has been long known to occur, and has been fully described by Maxwell Masters in his work on "Vegetable Teratology,"[1] under the heading of 'Regular Peloria' and 'Irregular Peloria.'

In the first variety irregular flowers become regular, as when a Violet drops its spurs and inequality of sepals and petals, and appears as a perfectly regular flower. On the other hand when the flower becomes regular by the increase in number of its irregular portions, as in the Toad-flax when it develops equal sepals, and five spurs instead of only one, this is known as irregular peloria. In this latter case Masters says "the peloria is evidently *not congenital*, but occurs at a more or less advanced stage of development."

Sometimes this kind of peloric change shows itself in all the flowers on a plant, while at other times a plant will be found with part normal and part peloric flowers. The phenomenon is wide-spread and is apt to occur in all kinds of plants. Masters says that changes of this kind are known to occur in no less than 110 plants, and he agrees with Darwin in thinking that in many cases such transformations cannot be atttributed to 'reversion.' Some of these peloric sports are moreover capable of reproducing themselves by seeds, as has been shown by Darwin and others.

Referring to facts of this kind, and some others to which I am about to refer, recorded by Darwin in the extremely interesting and important chapter xi. on 'Bud Variation' in his "Animals and Plants under Domestication," I find that so long ago as 1872 I expressed the following opinion[2]:—"We think, moreover, that Mr. Darwin fails to attach an adequate importance to such instances of 'spontaneous' variation as he has recorded. These instances would seem to afford most interesting examples of the operation in higher organisms of those influences which suffice to produce such multitudes of heterogenetic changes among lower organisms; so that they supply most valuable additional testimony

[1] Ray Society, 1859, pp. 219–239.

[2] "The Beginnings of Life," Vol. II, p. 595.

as to the continued influence of 'organic polarity' in determining the form and structure of higher organisms."

If looked at from these points of view, we shall be more fully able to appreciate the importance of many of the instances cited by Darwin. One of the simplest, and yet most satisfactory, examples is that recorded concerning the rare and occasional production of nectarines upon peach-trees, and the reverse. Speaking of the peach, Darwin says:—"This tree has been cultivated by the million in various parts of the world, has been treated differently, grown on its own roots and grafted on a stock, planted as a standard, against a wall, and under glass; yet each bud of each sub-variety keeps true to its kind. But occasionally, at long intervals of time, a tree in England, or under the widely different climate of Virginia, produces a single bud, and this yields a branch which ever afterwards yields nectarines. Nectarines differ, as every one knows, from peaches in their smoothness, size, and flavour; and the difference is so great that some botanists have maintained that they are specifically distinct. So permanent are the characters thus suddenly acquired, that a nectarine produced by bud-variation has propagated itself by seed."

Changes of this kind—and several others which Darwin records[1]—are doubtless due to some molecular modification, brought about in an unknown manner, in the tissues of the bud which varies; so that the production of the nectarine structure is the result of the altered balance and the new moving equilibrium which becomes necessitated in the tissues of the growing part. Such an explanation of these apparently 'spontaneous' changes in plants may be illustrated by alterations which are liable to occur in certain parts of animals when they are exposed to particular influences. Thus Wallace says[2]:—"The Indians (of South America) have a curious art by which they change the colours of the feathers of many birds. They pluck out those from the part

[1] Somewhat analogous changes, for instance, occasionally occur in rose-trees, whereby a moss-rose suddenly appears upon a tree belonging to a totally different variety (*loc. cit.*, pp. 379–381). And in regard to these changes it is important to bear in mind that many of them are certainly not to be attributed to 'reversion.' Thus Darwin says (*loc. cit.*, II, p 255):—"No one will maintain that the sudden appearance of a moss-rose on a Provence-rose is a return to a former state, for mossiness of the calyx has been observed in no natural species; the same argument is applicable to variegated and laciniated leaves; nor can the appearance of nectarines on peach-trees be accounted for with any probability on the principle of reversion."

[2] "Travels on the Amazon and Rio Negro," p. 294.

which they wish to paint, and inoculate the fresh wound with the milky secretion from the skin of a small toad. The feathers grow of a brilliant yellow colour, and on being plucked out, it is said, grow again of the same colour without any fresh operation." Although this change seems to be producible at will by a definite agent, we really know no more concerning the actual steps of the process by which it is produced, than we know concerning the intimate nature of the changes by which the peach-branch is metamorphosed into a nectarine-branch. The possibility of the occurrence of the molecular change which occasions this metamorphosis is, moreover, by no means limited to the buds of the peach-tree. For, as Darwin tells us, "nectarines have likewise been produced from the stone of the peach, and, reversely, peaches from the stone of the nectarine." And even this is not all, since "the same flower-bud has yielded a fruit one half or one quarter a nectarine, and the other half or three quarters a peach."

So much then for facts concerning 'sports' and 'discontinuous variation' which have been known for rather a long time. Let us look now to the more recent evidence tending further to show in an unmistakable manner that even in regard to higher forms of life the origin of new species may occur by sudden 'mutations' rather than by the slow cumulation through many generations of minute progressive variations.

In his recent important work Prof. de Vries seems to have certainly demonstrated this point in regard to the 'Evening Primrose' (*Œnothera Lamarckiana*) by means of observations carried on during ten years upon 50,000 of the descendants of some of these plants placed by him in the botanical gardens of Amsterdam. Preliminary experiments made with this and a number of other plants showed it to be favourable for such observations. Of the 50,000 descendants of this plant, it was found that "about 49,200 were in no respect different from the original patriarchal *O. Lamarckiana*, showing no tendency towards gradual change in any special direction, but only the common small fluctuating 'variations' as regards size and appearance on either side of a normal—in fact, resembling in that respect other plants and animals which are left to themselves without being interfered with."

"Quite otherwise with the 800 other plants. None of these, although appearing spontaneously, could be said to be representa-

tives of *O. Lamarckiana* from which they were descended. De Vries arranges them in seven distinct species, viz., 1 of *O. gigas*, 56 of *O. albida*, 350 of *O. oblonga*, 32 of *O. rubrinervis*, 158 of *O. Uanella*, 221 of *O. lata*, and 8 of *O. scintillans*."[1]

Then, again, the representatives of these new species produced descendants the majority of which unmistakably belonged to the same species as itself. Some of the new species like *O. gigas* are very stable forms. Thus from the one specimen of this plant "were obtained 450 plants, all of which, with one exception, were *O. gigas* —the one exception not being a return to *O. Lamarckiana*, but belonging to a new variety." On the other hand, *O. scintillans* is "extremely unstable, *i.e.*, possesses the property of mutation to a high degree, a large proportion of its descendants belonging to other species, especially *O. oblonga* and *Lamarckiana* itself."

All these facts concerning the "black-shouldered" Peacock, the Ancon sheep, the tall American maize, and the various species of Œnothera, are so many examples of transformations more or less rapidly brought about, not of course due to internal changes alone but to the conjoint influence of internal tendencies and external conditions operating at the time—the latter being an aspect of the problem which Weismann ignores, or he would never have made such a statement as this: "When by germinal metamorphosis a new form has arisen, this, from the first moment of its existence must be adapted to the new conditions of life or it mush perish." The cases above referred to would doubtless be regarded by him as cases of 'germinal' metamorphosis, and yet they have been so far adapted to their conditions of life, that instead of perishing they have increased, multiplied and perpetuated their kind.

Again, Prof. Weismann writes "the abrupt transformation of species implies sudden change in the conditions of life," to which one can only reply that it has not always been so; as evidence of which there has been on different occasions amidst the old conditions the appearance of the "black-shouldered" Peacock, and the appearance of new species of Œnothera. And when he goes on to say, "if such abrupt transformation takes place it must produce the new form instantly equipped for the struggle for existence, and adapted in all its organs and systems of organs to the special conditions of its new life," I rejoin that this does not in the least apply

[1] See "Nature," June 27, 1901, p. 208.

to the appearance, by comparatively abrupt transformations, of multitudes of the lower forms of life such as Amœbæ, flagellate Monads, Moulds, Peranemata, Ciliated Infusoria, and many other of the lower types which are by no means so dependent upon specialised external conditions, and amidst which the part played by 'natural selection' seems reduced to its lowest terms.[1]

Statements such as these are all that Prof. Weismann in the course of eleven pages was able to say against the occurrence of a *per saltum* development, as one of the modes in which Evolution is carried on.[2] His statements and arguments must be absolutely unconvincing to any open-minded critic.

So far from there being any *à priori* objections to *per saltum* development or Heterogenesis, which, as I shall claim to show, occurs so frequently, the fact of its occurrence ought to be considered as thoroughly harmonious with all that we know concerning allotropic and isomeric changes in simpler forms of matter, and concerning the constitution of living matter itself.

For the present, however, the subject of Heterogenesis is postponed, and I intend to limit myself to a brief account of the views of de Vries.

After pointing out that the origin of species has hitherto been considered an excessively slow process and one "withdrawn from actual observation, or at least from experimental treatment," he contends that now in accordance with his views the process should be regarded as much more rapid, and as one coming within the range of direct observation and experiment. He disbelieves in minute common variability as a starting point for natural selection and the production of new species, and maintains that such a process "does not lead by even the sharpest persistent selection, to any real transgression of the limits of species, much less to the origin of new and constant attributes."

"According to the Mutation theory species have not originated by gradual selection, continued through hundreds or thousands of years, but by sudden steps, even if the changes are very small. Unlike the variations which are progressive changes in a straight line, those metamorphoses which are designated as 'mutation' branch off in new directions. Furthermore, so far as

[1] In Chaps. ix-xiii. the origin of all these forms as a result of more or less "abrupt transformation" will be dealt with, while many other heterogenetic transformations are described in "Studies in Heterogenesis," 1904.

[2] See his "Studies in the Theory of Descent," Transln., 2 Vols. 1882, pp. 697–708.

experience goes, they occur at random—that is, in the most diverse directions."

"The new species is at once such, and originates from the former species without apparent preparation and without graduation. Each attribute, of course, arises from one previously present, not by their normal variation, but by one small yet sudden change. Provisionally one may compare these changes, but only in the simplest manner, with chemical substitution."

Then in regard to the Darwinian view that "species have originated by natural selection in the struggle for existence," he says, "whenever the contest occurs between individuals of one and the same elementary species, it also occurs between the different species as such. The first-mentioned contest pertains to the doctrine of variability, the second to that of mutation. In the first-mentioned case those individuals survive which find their life conditions most favourable, and they are therefore generally the most vigorous. By this process local races originate, and by it acclimatisation is made possible. If the new life conditions cease, then the adapted races revert to the original type. . . . Natural selection in the struggle for existence between the newly originated elementary species is quite different. These originate suddenly, unmediated, and multiply themselves if nothing stands in the way, because they are for the most part completely, or in a high degree, heritable. If then the increase leads to a struggle for existence, the weaker succumb and are rooted out. According as the older or the younger form happens to be the better suited for the life conditions will one or the other survive. . . . The contest decides which of them shall survive and which shall perish. These 'species selections,' in the course of their evolution, have, without doubt, rooted out immense numbers and retained only a small proportion. Briefly stated, I assert, of course on the ground of the mutation theory, that by the struggle for existence and natural selection species do not originate, but perish."

As a factor in Evolution, therefore, it is clear that the 'mutation' theory of de Vries is intended by its author actually to replace the theory of 'natural selection.' To what extent it will do so cannot be decided till much additional work has been done and many years have elapsed. In many respects his views are in accord with those of Cope, Packard, and Hyatt already referred to.

De Vries believes that all species and genera, while always subject to the full range of fluctuating variability, exist at

times in a mutable and at times in an immutable condition, the latter state being very much the more prevalent. Indeed so prevalent is the immutable condition among plants that only a small proportion of the species embraced in the flora of any given region may be found existing in their mutable period. Furthermore, it has been found, and was to be expected, that some plants, even when in the fulness of their mutable period, would exhibit their mutability more readily than others.[1]

At the conclusion of an address delivered last year before the University of Chicago, which the author has kindly sent me, Prof. de Vries said :—"Mutability is manifestly an exceptional state of things if compared with the ordinary constancy. But it must occur in nature here and there, and probably even in our own immediate vicinity. It has only to be sought for, and as soon as this is done on a sufficiently large scale the study of the origin of species will become an experimental science."

All that is said in the last two paragraphs will subsequently be found to be just as applicable to Heterogenesis as it occurs among lower forms of life, except that the resulting transformations are then much more radical and complete than are those resulting from 'Mutation.' In these latter changes, when occurring in higher plants, each newly originated species, while possessing distinctly separate attributes, is never, as de Vries intimates, very widely different from the parent form.

The contrast between the two processes above referred to and those of Natural Selection is great in all ways, seeing that the latter never admits of actual proof and observation, depending, as it does, or is commonly believed to do, upon the progressive accumulation of most minute useful variations along a particular line, through generation after generation, so that it leads not to random or purposeless features, but always to "ceaseless adaptations of the species to its life-conditions." The time needed for the production of all the forms of life that have ever lived by a process so essentially slow as this would surely be prodigious, and must inevitably far exceed anything that geologists would demand—not to speak of the far narrower limits which physicists are willing to assign for the age of our globe, dating from the consolidation and cooling of its surface.

[1] This account of the views of de Vries is derived from an interesting paper by Charles A. White, in "The Smithsonian Report" for 1901, pp. 631–640.

CHAPTER VII

OTHER FACTORS OF EVOLUTION

IT is important to bear in mind what has hitherto been said as to the comparatively trivial character of the differences that mostly go to separate one species from another, and the common absence of any evidence of 'usefulness' in such characters. But if we regard 'organic polarity' and changes in molecular constitution as lying at the root of the origin of 'sports' and as the cause of 'mutation' generally, there would no longer be any discrepancy in many cases, since we are expressly told by de Vries that the changes due to mutation "occur at random—that is, in the most diverse directions."

In illustration of the comparative ease with which changes are brought about in the specific characters of some organisms, reference may be made to the results of experiments with the larvæ of certain butterflies. It is now well established that the colours and markings of many of these latter, together with the size, and to a less extent, the shape of their wings, are largely dependent upon the conditions as regards temperature and light under which the caterpillars and pupæ have been reared. Hence the phenomena of 'Seasonal Dimorphism,' in which one form of butterfly is bred in the Spring, and a different one later on in the Summer. But the experiments of Merrifield[1] and of Weismann have shown that one of the two seasonal forms may be bred from the larvæ of the other form by simply altering the temperature under which the larvæ are reared—the changes, in fact, though slighter in amount, are produced just in the same way that changes in the colour and forms of crystals are brought about (pp. 45, 63).

There are also other sets of changes of like kind, lying outside the pale of utility, and yet apparently connected with one another in some mysterious manner : I allude to those which were grouped

[1] "Nature," Dec. 23, 1897, p. 184.

by Darwin under the head of "Correlated Variability."[1] In reference to this subject he says, "Correlated variation is an important subject for us; for when one part is modified through continued selection, either by man or under nature, other parts of the organisation will be unavoidably modified." This is thoroughly in harmony with what might be expected in accordance with H. Spencer's doctrines. Thus he says[2]:—"if along with a striking change in a flower which the florist contemplates, there go changes all over the plant not obvious to careless observers but visible to him, we must infer that there are everywhere minute differences which even the florist cannot perceive: the whole constitution of the plant has diverged in some measure from the constitution of kindred plants. Every local variation implies a change pervading the entire organism, manifested in concomitant variations everywhere else. . . . If so, what becomes of the hypothesis of determinants—the hypothesis that there is a special element in the germ-plasm which results in a special local modification in the adult animal?" Yet that the facts are as H. Spencer states he shows by three quotations, one from Darwin and two from Dr. Maxwell Masters. One of the latter refers to the point that among seedling 'stocks' which have not yet flowered, those that will produce double flowers are already distinguishable. Dr. Masters says:—"This separation of the single from the double-flowered plants, M. Chatié tells us is not so difficult as might be supposed. The single stocks, he explains, have deep green leaves (glabrous in certain species), rounded at the top, the heart being in the form of a shuttlecock, and the plant stout and thick-set in its general aspect; while the plants yielding double flowers have very long leaves of a light green colour, hairy and curled at the edges, the heart consisting of whitish leaves, curved so that they enclose it completely."

It is certainly true that while such facts as these are wholly incongruous with the hypothesis of determinants, they are in accord with the hypothesis of physiological units. As H. Spencer says, "that a change of structure arising in one part of the organism is accompanied by multitudinous changes of structure in other parts of the organism, is not only congruous with the belief that there exist such constitutional units, but yields it distinct support."

[1] "Animals and Plants under Domestication," 1868, Vol. II, Chap. XXV.
[2] *Loc. cit.* Vol. II, p. 622.

In order, however, to account for all the facts concerning 'sports' of all kinds, 'mutation' and 'correlated variability,' to which we have just been referring, as well as to many other difficulties besetting his exclusive doctrine concerning the all-sufficiency of 'Natural Selection,' Prof. Weismann relies upon a supplemental hypothesis which he calls 'Germinal Selection.' In addition, he appeals to it to explain the existence of non-adaptive characters in animals and plants; as a means of starting and increasing incipient variations till they can be made use of by natural and sexual selection; as a mode of accounting for the influence of new external conditions in modifying organisms; as a means of bringing about the diminution and even total disappearance of disused organs; and finally as yielding an "explanation of every heritable variation."[1]

In the last chapter we have seen how devoid of all foundation in fact is his doctrine of the 'Continuity of the Germ Plasm,' and what an extraordinary web of assumptions and hypotheses he has made use of, in conjunction with this basal doctrine, in his attempts to explain many known phenomena. And now all outstanding difficulties, he seems to think, are to be explained away by his supplemental hypothesis of 'Germinal Selection'—that is, by a process of selection occurring between the determinants of each 'id' and going on in all of them simultaneously.

In reference to this doctrine we have to bear in mind that the cell itself is a body of microscopic minuteness; that the bulk of the nucleus is very much smaller still; that the nucleus contains several minute chromosomes; that each chromosome is made up of a number of granular bodies termed ids (only visible with very high powers of the microscope); and that each granular id is assumed to be composed of many thousands of imaginary bodies named determinants—and that it is between these determinants that the nutritional struggle, or 'selection,' is taking place.

It is further assumed by Weismann that the rate of growth of these imaginary determinants within the granular id will depend "mainly on the amount of nourishment which reaches them"; and that there will be "unequal nutrition of the determinants conditioned by the chances of the food-supply." Moreover, owing to their different assimilative powers "while one determinant may be slowly becoming weaker, its neighbour, on the other hand, may

[1] See "The Evolution Theory," Vol. II, pp. 113–158.

be varying on an ascending scale, just because the former is, on account of its diminished power of assimilation, no longer able to exhaust completely the food stream which flows to it"—that is, within the very minute microscopic granular id. Some of these assumed changes in rate of growth or nutritive vigour are believed to be 'spontaneous,' while others are 'induced' by changes in external conditions. Weismann's doctrine necessitates, however, the consideration of these other additional points:—"Each id contains potentially the whole organism, though with some individuality of expression. The child is thus not determined by the determinants of a single id, but by those of many ids, and the variations of any part of the body do not depend upon the variations of a single determinant X, but on the co-operation of all the determinants X which are contained in the collective ids of the relevant germ-plasm. Thus it is only when a majority of the determinants have varied upwards or downwards, that they dominate collectively the development of the part X and cause it to be larger or smaller."

It is not only, he tells us, that the determinants may "become larger or smaller as a whole, but some kinds of the 'biophors' of which they are made up may increase more than others under definite altered conditions, and in that case the determinants themselves will vary qualitatively . . . consequently also qualitative variations of the organs controlled by the determinants—the determinates," will be caused.[1]

Many persons will probably fail to find much security in these complicated considerations—especially when they bear in mind that the foundations on which they are based lie among shifting and delusive sands rather than upon a bed of solid rock. H. M. Vernon believes this hypothesis of 'Germinal Selection' to be absolutely at variance with an important body of well-established facts. Thus he says,[2] "This theory, though plausible enough, is absolutely opposed to fact in so far as it relates to the evolution of more adaptive forms . . . it is no more than an hypothesis, which has not, and never can have, any experimental evidence to support it." Haeckel also rejects it. He says:—"We may admire the subtlety and depth of the speculations with which Weismann has worked out his elaborate molecular theory. But the nearer we get to its foundations the less solid we find

[1] *Loc. cit.*, II, pp. 125 and 152.
[2] "Variation in Animals and Plants," 1903, p. 396.

them. Moreover not one of the many supporters of the theory of germ-plasm has been able to make profitable use of it in the twenty years since it was first published. On the other hand it has had an evil influence in so far as it denied the inheriting of acquired characters, which I hold, with Lamarck and Darwin, to be one of the soundest and most indispensable supports of the theory of descent."[1]

There remain now for brief consideration three other factors of Evolution, namely, (1) Sexual Selection, (2) the Effects of Use and Disuse, and (3) the direct Influence of External Conditions; and in regard to each and all of them the degree of their influence as factors hinges upon the all-important question which we shall presently have to consider; that is whether Acquired Characters are or are not inherited.[2] This latter question has long been debated in relation to the last two of the factors just mentioned, but it has only recently been very seriously considered in reference to Sexual Selection. For this and other reasons it will be convenient briefly to consider and make some statements in reference to this factor before dealing with the other two.

In Darwin's view the process of Sexual Selection was essentially similar to that of Natural Selection, in fact, a corollary therefrom, and consequently explicable, with the necessary variations, in much the same kind of way. This view, however, has not been generally accepted. It has been rejected by Wallace, for instance, who instead of dwelling upon contests between males and the effects of natural selection, combined with heredity, in perpetuating the characters of the victors—whether in the direction of defence or of allurement—considers that the principal cause of the greater brilliancy of male animals in general, and of male birds in particular, is, that they do not stand so much in need of protection by concealment as the females do. For the rest he relies in the main simply upon Natural Selection.

Several other theories in connection with this subject are discussed by J. T. Cunningham in his interesting work "Sexual

[1] "The Wonders of Life," Transln., 1904, p. 104, see also pp. 385 and 391.

[2] In many collateral respects also this question is one of enormous importance, as may be gathered when Herbert Spencer says: "More than once I have pointed out that, as influencing men's views about Education, Ethics, Sociology, and Politics, the question whether acquired characters are inherited is the most important question before the scientific world" ("Principles of Biology," I, p. 672).

Dimorphism in the Animal Kingdom,"[1] but I must confine myself here to a brief account of his own views on this subject, which are strongly expressed, and definitely Lamarckian in character.

He says (*loc. cit.* p. 30), "The fundamental objection to Darwin's theory of sexual selection, or any other selection theory, is that it does not account for the origin of the variations which it assumes . . . there are two almost universal peculiarities of secondary sexual characters on which the theory of sexual selection throws no light whatever: (1) the characters do not begin to appear in the individual until it is nearly adult and sexually mature, in other words they appear when, or a little before, the animal begins to breed; (2) they are inherited only by the sex which possesses them," or, in other words, only by the members of one sex.

Unisexual characters have, as a general rule, some function or importance in the special habits, or conditions of life, of the sex in which they occur; whether they are fighting weapons like the antlers of stags; allurements as in the special plumes of male birds which are erected and displayed in courtship; or love-notes as in the songs of birds generally. "So far," Cunningham says, "there can be no doubt that Darwin was perfectly right and his opponents all wrong. The facts being so, there is rivalry, combat, and competition. But the important truth which appears to have been generally overlooked, is that in the case of each special organ its special employment subjects it to special, usually mechanical, irritation or stimulation, to which other organs of the body are not subjected." Such constantly repeated irritations or stimulations are causes thoroughly well recognised as being capable of producing physiological effects on the tissues of a distinct kind, and have been much dwelt upon by E. D. Cope in his important work, "The Primary Factors of Organic Evolution."[2] So that, as Cunningham says, "not only hypothetically at some former time, but actually at present in every individual, the unisexual organs or appendages are subjected in their functional activity to special strains, contacts, and pressures, that is, to stimulation, which must and does have some physiological effect on their development and mode of growth."

It is quite evident that the development of these 'physiological

[1] 1900, pp. 24-44. [2] 1896, Chicago.

correlations' at the present stage of evolution takes place by heredity, even though the usual irritations are partly or entirely wanting. If, therefore, we were at liberty to assume the inheritance of acquired characters (as to which there is so much dispute) the explanation of the existence of secondary sexual characters, and their strict limitations to a particular sex and a particular period of life, and often to a particular season, would present less difficulties than those which beset the explanations of Darwin and Weismann.[1]

Believing as he does in the inheritance of acquired characters the explanation of Secondary Sexual Characters adopted by Cunningham is this :—"*that the direct effects of regularly recurrent stimulations are sooner or later developed by heredity, but only in association with the physiological conditions under which they were originally produced*, and that this is the explanation of the limitation of particular modifications, not merely to particular species or kinships, but to particular periods in the life of the individual, to a particular sex, and even to a particular season of the year in that sex." He contends, indeed, that the whole series of multitudinous facts agrees remarkably with the hypothesis that secondary sexual characters are due to the inheritance of acquired characters. It is clear that until this latter doctrine is established —a time which we may hope is not far distant—Cunningham's view concerning secondary sexual characters will stand little chance of being accepted.

The evidence in regard to this subject of the Inheritance of Acquired Characters must now be briefly considered, as it is the all-important question in relation to Use and Disuse, and the Direct Influence of External Conditions. If the effects of these influences are confined to individuals and not transmitted to their progeny, neither of them could be regarded as a factor of Evolution; while, on the other hand, if the effects acquired in either of these ways are capable of being transmitted to offspring it is clear that we should have in each of them a factor of Evolution of great importance.

Doubts on this subject were first started by Weismann in 1883

[1] Cunningham says (*loc. cit.* p. 40), "To my thinking the suppression of male characters in consequence of castration is in itself sufficient to disprove the theory of the absolute continuity of the germ-plasm, and its absolute independence of the somatic cells."

and were strongly reinforced two years later by the publication of his essay on "The Continuity of the Germ-Plasm."[1] In a later essay in the same volume, he says (p. 399):—"I think it is now generally admitted that a very important problem is involved in this question, the solution of which will contribute in a decisive manner towards the formation of ideas as to the causes which have produced the transformation of species. For if acquired characters cannot be transmitted, the Lamarckian theory completely collapses, and we must entirely abandon the principle by which alone Lamarck sought to explain the transformation of species,—a principle of which the application has been greatly restricted by Darwin in the discovery of natural selection, but which was still to a large extent retained by him." Weismann came to the conclusion that acquired characters cannot be transmitted, because, as he thought, there were no proofs of such transmission, and because its occurrence was theoretically improbable, or even 'inconceivable,' as some of his followers say.[2]

It is well never to lose sight of the fact that opposition to the view that functionally acquired characters are inherited is essentially associated with Weismann's doctrine of the 'Continuity of the Germ-Plasm,' and the other hypotheses founded thereupon. We have seen (pp. 76–81) how void of foundation in fact this doctrine is —and how much it is actually opposed to multitudes of facts which have been made known since the time when it was first enunciated. As a consequence, his sequential doctrine that changes undergone by the cells and tissues of the body generally are unable to produce any effect in the transformation of species, "simply because they can never reach the germ-cells from which the succeeding generation arises," becomes robbed of its principal support. And as to the alleged improbability, or inconceivability, of any means by which the germ-cells may be specifically altered as a result of changes taking place in the soma by use and disuse, or by the

[1] See his "Essays on Heredity," Transln., 2nd Edn., 1891, Vol. I, pp. 67 and 163.

[2] In the early days of this discussion far too much importance was attached to the fact that mutilations are not inherited. But these occur for the most part once only in the life of an individual, and are mostly experienced by one sex only, so that it is as Cope says (*loc. cit.* p. 400): "quite unreasonable to cite the history of mutilations as evidence against *the inheritance of natural characters produced by oft-repeated and long continued natural causes*." Still, some well authenticated cases of the inheritance of mutilations have been recorded by him (p. 431).

direct influence of conditions, one might have thought that the last persons to start such an objection would be Weismann and his followers, looking to the extraordinarily complex mixture of assumptions and hypotheses by which their own attempted explanation of heredity is sought to be supported.

Some of Herbert Spencer's last words on this subject may well be quoted here. In his "Facts and Comments" (1902, p. 92) he says:—"The doctrine of use-inheritance is rejected because of inability to 'conceive any means' by which a modification produced in an organ, can produce a correlated modification in the germ of a descendant. Yet the alternative hypothesis is accepted notwithstanding a kindred inability which is certainly not less and may be held much greater. If Weismann's view is true, such a structure as a peacock's tail feather implies over 300,000 determinants. Multiply that by the number of such feathers, and add those of the body feathers, as well as those of all the parts of all the organs, and then imagine the number of determinants which must be contained in the microscopic sperm-cell. Further imagine that in the course of the developmental transformations, each determinant finds its way to the place where it is wanted! Surely to 'conceive any means' by which these requirements may be fulfilled, is not a smaller difficulty if it is not a greater," than that which has to be met in connection with the doctrine of 'physiological units.' [1]

The question of relative conceivability between Weismann's doctrine of absolutely innumerable determinants in the germ-plasm, destined to produce corresponding parts in the offspring, and Spencer's doctrine of physiological units cannot by any possibility be said to be easier for the former doctrine; and when one considers how this doctrine utterly fails to explain multitudes of known facts concerning repair and regeneration in animals, and reproduction by buds, cuttings and superficial cells in plants, with which Spencer's doctrine is quite in harmony, there ought not to be room for doubt as to which hypothesis is most in accord with facts. And in reference to the conceivability of this latter doctrine,

[1] The above is an under statement of the case as to the conceivability of Weismann's views, because the "sperm-cell" contains a number of chromosomes, and each of these bodies is made up of a number of granules—the ids—and each one of these granules is supposed to contain the total number of determinants referred to above, for the building-up of the body, which Spencer speaks of as contained in the entire "sperm-cell."

as Spencer says elsewhere,[1] "Should it be said that such a process is too marvellous to be reasonably assumed, the reply is that it is not more marvellous than heredity itself, which, were it not familiar to us, would be thought incredible." In regard to the general question he had previously said[2]: "At last then we are obliged to admit that the actual organising process transcends conception. It is not enough to say that we cannot know it; we must say that we cannot even conceive it: can only conceive the possibility of a suggested interpretation."

Setting aside all prejudice, therefore, and with only a warrantable leaning towards the doctrine which so far has been found to be most congruous with the facts, it only remains to look at some of the actual evidence tending to support the view that the Effects of Use and Disuse, and also the direct Effects produced by altered External Conditions can be, and are frequently, transmitted to offspring.

The effects of Use and Disuse are effects that would be met with in animals rather than in plants, and apart from secondary sexual characters to which reference has already been made, the discussion in recent years has largely been confined to questions touching the effects of use-inheritance on the frame of man himself.

It is well known that Darwin[3] collected a number of facts implying that functionally altered structures are transmitted by heredity. Among the facts that he adduced, he showed especially that there is a changed ratio between the wing-bones and the leg-bones in the domesticated as compared with the wild duck, and that alterations in the bones of fowls and rabbits had also taken place. Concerning such changes he said:—"From the foregoing facts there can be no doubt that certain parts of the skeleton in our anciently domesticated animals have been modified in length and weight by the effects of decreased or increased use." He also attached much importance to the increased or diminished length of the intestines, resulting apparently from changed diet. It has been found, for instance, that the intestines of the domestic cat are one-third longer than those of the wild cat of Europe, and Darwin adds:—"The increased length appears to be due to the domestic

[1] "Principles of Biology," II, Appendix G.
[2] *Loc. cit.*, II, p. 621.
[3] "Animals and Plants," 1868, II, pp. 295–303.

cat being less strictly carnivorous in its diet than any wild feline species; I have seen a French kitten eating vegetables as readily as meat. According to Cuvier the intestines of the domesticated pig exceed greatly in proportionate length those of the wild boar. In the tame and wild rabbit the change is of an opposite nature, and probably results from the nutritious food given to the tame rabbit."

Turning now to examples more recently made known I may say that the transmission of acquired characters among Bacteria is sometimes cited in this connection. Facts of this kind could be multiplied to almost any extent,[1] but seeing that these organisms possess no nucleus, and, for the most part, multiply merely by fission, we have here only to do with 'discontinuous growth,' and a process lying at the root of heredity rather than with heredity itself. The very similar facts that have been made known by Klebs, L. Errera and others as to the transmission of acquired characters in lower Fungi are, however, more worthy of being cited. One of the observations by Errera will be found recorded by Spencer;[2] while other important observations of the same kind have been published by George Massee in a recently issued number of the 'Philosophical Transactions' (197, B), showing that in the course of from twelve to sixteen generations several ordinary saprophytic Fungi could be made to take on a parasite habit, and perpetuate themselves as parasites.

Haeckel regards the spiral shape of the shells of Gasteropods as "a very fine instance of the inheritance of acquired characters." He says the snail's house "is in essence a spirally coiled tube, closed at the upper end and open at the lower (or mouth): the mollusc can at any moment withdraw into its tube. The comparative anatomy and ontogeny of the snail teach us that this spiral cell came originally from a simple discoid or cylindrical dorsal covering of the once bilaterally-symmetrical mollusc, by the two sides of the body having an unequal growth,"[3] the particulars of which he then traces. Haeckel likewise regards the asymmetrical shapes of flat-fishes, such as soles, flounders, and turbots, as results of changes that they have gradually acquired through many generations owing to their habit of lying on one side (right or left) at the

[1] Some good examples have recently been cited by Prof. Adami, of Montreal, in the "Brit. Med. Jrnl.," May 29, 1905, p. 1135.

[2] "Biology," II, p. 623.

[3] "The Wonders of Life," p. 185.

bottom of the sea, though they are well known to have a perfect bilateral symmetry when young, like that of ordinary fishes. Cunningham also attaches much importance to these changes. Thus he says :—"The theory of independent variation and selection as applied to flat-fishes is unsupported by evidence, while the conclusion that the metamorphosis of these fishes is the direct result of the change of conditions is in harmony with all that we know of the effect of physical conditions on individual organisms."[1] He cites also another remarkable case in which particular habits seem to have engendered a definitely corresponding change in another fish (Anableps) living in the estuaries of Brazil and Guiana, which "does not wear spectacles, but actually has its eyes made in two parts, the upper half of the lens having a different curvature from that of the lower. The pupil is also divided into two by prolongations from the iris." And the explanation of this remarkable condition seems to lie in the fact that, "this fish is in the habit of swimming at the surface with its eyes half out of the water, and the upper half of the eye is adapted for vision in the air; the lower half for vision under water." There is no reason, he says, to suppose "that the required variations ever occurred until the ancestors of Anableps took to swimming with their eyes half out of water" (*loc. cit.* p. 16).

Haeckel likewise maintains that the history of parasites "provides an abundance of the most striking proofs of the much-contested inheritance of acquired characters."[2] No other circumstance has so profound an influence on the organism as adaptation to a parasitic existence. Moreover, as he adds, "there is no other section in which we can follow step by step the course of the degeneration which is caused, and show clearly the mechanical nature of the process." This may be fully admitted without denying the share that Natural Selection often takes in the process.

These cases ought rather to be referred to under the next head of changes wrought by alteration in the conditions of life. As examples occurring in animals, however, it is more convenient to refer to them here, and also to two other cases showing the remarkable changes that may be brought about in some of the embryos of bees and ants by mere variations in the food with which they are supplied. There is first the well-attested fact of

[1] *Loc. cit.* p. 23.
[2] "The Wonders of Life," p. 243.

the production of a queen bee rather than a neuter according to the kind and amount of food given to the larva; and secondly, in a communication quoted by H. Spencer (I, p. 687), from the Superintendent of the Royal Botanic Gardens in Trinidad concerning what are known as 'Parasol' ants, it is said that by different modes of feeding "ants can practically manufacture at will, male, female, soldier, worker, or nurse"; while the same authority adds, "Some of the workers are capable of laying eggs, and from these can be produced all the various forms as well as from a queen's egg."

We must turn now to the different cases cited by Herbert Spencer during the celebrated discussion that took place between him and Weismann in the pages of *The Contemporary Review* in 1893–4, his contributions to which are reproduced as an Appendix to the first volume of "The Principles of Biology." An admirable *résumé* of his own point of view will be found in his fourth article (pp. 671–695), so that only brief references to the principal instances cited will be mentioned here. One of the most weighty has reference to the co-adaptation of co-operative parts in reference to use-inheritance. He cites, for instance, the case of the extinct Irish elk, the male of which had enormously developed horns, weighing upwards of a hundredweight, "carried at great mechanical disadvantage: supported as they are along with the massive skull which bears them, at the extremity of the outstretched neck." Moreover, "that these heavy horns may be of use in fighting, the supporting bones and muscles must be strong enough, not simply to carry them, but to put them in motion with the rapidity needed for giving blows. Let us then ask how, by natural selection, this complex apparatus of bones and muscles can have been developed *pari passu* with the horns? . . . It would be a strong supposition that the vertebræ and muscles of the neck suddenly became bigger at the same time as the horns. It would be a still stronger supposition that the upper dorsal vertebræ not only at the same time became more massive, but appropriately altered their proportions, by the development of their immense neural spines. And it would be an assumption still more straining our powers of belief, that along with heavier horns there should spontaneously take place the required strengthenings of the bones, muscles, arteries, and nerves of the scapula and the fore-legs" (pp. 537–8). Through the intermediation of the inherited effects produced by use all the co-operative parts could

vary together ; on the other hand it is impossible to imagine how such correlated variations in different parts could spring up spontaneously and be developed by natural selection.

Then, again, there is the case of the rudimentary hind limbs of the whale, as to which Herbert Spencer says, "though during those early stages of decrease in which the disused limbs were external, natural selection probably had a share in decreasing them, since they were then impediments to locomotion, yet when they became internal, and especially when they had dwindled to nothing but remnants of the femurs, it is impossible to suppose that natural selection played any part : no whale could have survived and initiated a more prosperous stirp in virtue of the economy achieved by such a decrease." It is surely, as he says, much more reasonable to attribute this extreme dwindling to the prolonged effects of disuse perpetuated through innumerable generations.

The effects of use and the exercise of a particular function through many generations has been well shown by Prof. Brewer of Yale University in his account of the "evolution of the trotting horse." He says fast trotters were not wanted until the improvement in roads and in wheeled vehicles during the last quarter of the eighteenth century "caused an increasing demand for fast roadsters for light draft." An authority on this subject, writing in 1796, considered that the utmost speed of an English trotter (supposed to excel all others) to be a mile in two minutes and fifty-seven seconds. During rather more than the next twenty years, both in England and in America, a few of the best animals "trotted a mile in three minutes," but none in less than the time above given. After this period trotting "became a recognised sport under specific rules," and "trotting records" began to be regularly kept. The horses too were carefully bred, so that after a time "heredity began to tell." By 1868 the speed had very notably increased, so that "there were several horses in the 2:20 class, and nearly one hundred and fifty in the 2:30 list ;" while by 1892, "there were 5,908 in the 2:30 list, 507 in the 2:20 list, and seven in the 2:10 list." [1]

We come next to a number of characters presented by man himself which seem to exemplify changes resulting from the inheritance of functionally produced modifications. There is, for instance, the varying powers of tactile discrimination possessed by the skin

[1] Quoted by Cope in "The Primary Factors of Organic Evolution," p. 426.

and mucous membranes in different parts of the body, and especially the extreme sensitiveness of the finger-tips and of the tongue; the diminution of the size and weight of the jaws and teeth which characterises the civilised races, as contrasted with the savage races; and the simultaneous increase in size of the great toe and diminution of the little toe, as the result of a cause "which has been operating ever since the earliest anthropoid creatures began to decrease their life in trees, and increase their life on the earth's surface," the changes being attributable to the fact that "effort is economised and efficiency increased in proportion as the stress is thrown more and more on the inner digits of the foot and less and less on the outer digits." Dr. Havelock Charles has also made known more than twenty differences, chiefly in the structure of the knee and ankle joints, to be met with between the leg-bones of Europeans and those of the Punjaub people—"differences caused by their respective habits of sitting in chairs and squatting on the ground." Markings and facets possessed by the latter and transmitted by heredity (since they are to be found in the new-born infant and even in the fœtus) are present owing to their habit, uniformly persisted in through innumerable generations, of squatting; while there is a total disappearance of the markings in question in the skeleton of the European, "as no advantage would accrue to him from the possession of facets on his bones fitting them for postures not practised by him" (*loc. cit.* I, p. 689).

These points noted by H. Charles are, however, only special instances of what had previously been established by Cope. He says (*loc. cit.* p. 467):—"all the form characters of the vertebrate skeleton, and for that matter, of the hard parts of all animals, have been produced by muscular pressures and contractions, and the friction, strains, and impacts due to these . . . the characters of the skeleton can generally be shown to be inherited, because they appear before birth, and are found at some stage or another of fœtal life." And elsewhere (p. 9) he says:—"By the discovery of the paleontologic succession of modifications of the articulations of the vertebrate, and especially mammalian skeleton, I first furnished an actual demonstration of the reality of the Lamarckian factor of use or motion, as friction, impact, and strain, as an efficient cause of evolution." He believes this kind of evidence to be also particularly strong in regard to the formation of the segments of the body and limbs of the Arthropoda (p. 404).

Then, again, when gout is acquired by the individual, a predisposition thereto may undoubtedly be transmitted to offspring. The same holds true of epilepsy; and when great musical talent or great powers of calculation show themselves, such powers are also apt to be transmitted to some of the progeny. In these latter cases especially it seems much more reasonable to suppose that the unusual powers have taken *origin* in the soma rather than in the germ-plasm—that is to say, that the individuals in whom such unusual powers first show themselves owe them to developmental changes that have taken place in particular parts of the brain in the ordinary course of things. The development of different regions of the brain is subject to constant variations in all individuals. One person has highly developed auditory centres and cerebral regions in association therewith, another has a poor development of the same parts; and the same thing holds good for the visual centres and their associated cerebral mechanisms. Some of those having highly-developed auditory centres may prove to have unusual musical abilities; while others, like Inaudi, may have marvellous powers in dealing with figures. His extraordinary capacity in this direction showed itself first when he was only six years old, and has continued ever since. He said to one who interviewed him a few years since, "Reading or writing figures is of little aid to me. I *must hear them.* I have no more than an average memory for colours, forms, events, places or musical airs. Although I cannot remember for any length of time a couple of lines of poetry, I can easily remember without any effort a long series of figures, varying from 25 to 30 lines, after I have only heard them once." Other calculators are known to depend upon visual rather than upon auditory symbols; while one of them, George Bidder, F.R.S., who as a youth was known as the "Calculating Boy," has certainly transmitted his powers to some of his descendants, though to a minor extent.

Some striking facts, illustrating the development of certain cerebral powers by use and practice carried on through generation after generation, have recently been cited by W. Woods Smythe[1] who says: "Lately I heard a missionary at a May meeting tell of the marvellous facility with which Chinese children memorise whole books of the Bible; the four Gospels, and sometimes the Acts also, being an easy feat for children of ten or twelve years. Having care-

[1] "Nature," June 15, 1905, p. 152.

fully sought information, from other authorities, I find these facts confirmed, and that the same applies to Mohammedan children. We are aware that for ages their ancestors have been compelled to memorise long portions of their sacred books, and although occasionally we meet with a child of any nation with a gigantic memory, that differs widely from the case of a people where it has become a general characteristic."

Another point, depending upon cerebral development in particular directions and its transmission by heredity, is also of much importance. A child usually learns to speak slowly and with much difficulty; but I have contended elsewhere[1] that the child would never be able to speak at all if he had not inherited from very remote ancestors certain nervous mechanisms in the brain upon which this power depends; and that if the child has to learn to speak this is due to the fact that he begins to try as soon as the appropriate nerve centres begin to develop. When, in certain cases, the acquirement of speech has not taken place at the usual time, it sometimes happens that years after, on the occurrence of some strong stimulus the previously dumb person begins to talk, without going through the usual process of learning how to do it. Four cases in illustration are cited. One was a patient of my own, one a patient of the late Dr. Wigan, and the other two are matters of ancient history—cases referred to by Herodotus, which, in face of the modern instances, cannot be lightly dismissed.

Herbert Spencer, as is well known, holds that the inheritance of functionally-caused alterations has come "more to the front as evolution has advanced," and that "it has played the chief part in producing the highest types . . . the action of natural selection being limited to the destruction of those who are constitutionally too feeble to live, even with external aid. . . . Natural selection acts freely in the struggle of one society with another; yet among the units of each society, its action is so interfered with that there remains no adequate cause for the acquirement of mental superiority by one race over another, except the inheritance of functionally-produced modifications."[2]

The question whether Acquired Characters resulting from changes in the External Conditions of Life are inherited or not cannot be profitably discussed without precise references to Weis-

[1] "Aphasia and other Speech Defects," 1898, pp. 5–8.
[2] "Biology," I, pp. 553 and 560.

mann's doctrine, so as to bring out the real difference between it and that of those who incline to the Lamarckian view. His doctrines will be found very distinctly stated in one of his "Essays upon Heredity,"[1] though scattered passages in his recent work, "The Evolution Theory" will help to clear up doubtful points.

New characters in plants or animals are, he holds, either (*a*) *blastogenic* and transmissible, or (*b*) *somatogenic* and non-transmissible. Modifications of the latter type due to reaction of the soma under altered external influences are regarded as alterations limited to the individual, because they are supposed to leave the germ-plasm unaffected.

The new characters, then, that can be transmitted to progeny are those only that are correlated with and due to *blastogenic* changes (or changes in the germ-plasm). There is, however, nothing distinctive in Weismann's point of view here. It is a doctrine which is taught also by Herbert Spencer, and by those who agree with him.

The real difference which divides the two schools is as follows. According to Weismann changes in the soma cannot lead to, or be associated with, definite and correlated changes in the germ-plasm; though changes in the germ-plasm are in some inscrutable way, through the intermediation of his countless armies of determinants and biophors, supposed by him to be capable of leading to precise correlated changes in the soma. According to Spencer and others, however, changes in the soma induced by external conditions *may* become associated with definitely related changes in the germ-plasm. Such correlated changes are held by them to be no more impossible or inscrutable than those which we see every day actually occurring in the reverse order, namely in the development of animals and plants from their germs: although in view of the excessively complicated nature of the processes, "we are obliged," as Spencer says, "to admit that the actual organising process transcends conception."

Thus, in the opinion of Weismann, the soma cannot communicate precise changes to the germ-plasm, though the germ-plasm can lead to an exact building-up of the soma; while according to Spencer the germ-plasm and the soma are mutually capable of influencing one another in specific ways.

[1] Edition of 1891, pp. 423–428 especially.

We must now see how far Weismann is consistent in holding his doctrines, and to what extent the logic of facts may have compelled him to make admissions, which practically bring him almost into accord with Spencer and his followers.

The *blastogenic* changes with which heritable characters are correlated are, in the opinion of Weismann, due either (1) to artificial or to natural selection, or else (2) to 'spontaneous' variations of the germ-plasm, from whatsoever cause arising.

In considering these views we must first come to some understanding as to the starting points for artificial and natural selection. According to Darwin,[1] "selection does nothing without variability, and this depends in some manner on the action of surrounding circumstances on the organism." On this same subject Weismann says,[2] "The ordinary, never-ceasing, always active germinal selection depends, we must assume, upon intragerminal fluctuations of nutrition, or inequalities in the nutritive stream which circulates within the germ-plasm." Such changes, however, he thinks, "have their roots in external influences." He asks, indeed, "how could the germ-plasm be changed except by the operation of external influences, using the words in their widest sense?" ("Essays," I, p. 424). And in his recent work he says (II, p. 137) when speaking of some of the influences of external conditions: "I call this form of germinal variation 'induced' germinal selection, and contrast it with 'spontaneous' selection, which is caused not by extra-germinal influences but by the chances of the intra-germinal nutritive conditions."[3]

Thus it is, in part, these 'induced' changes in the ids of the germ-plasm that, according to Weismann, produce the "variability" without which, as Darwin said, "selection does nothing." This direct action of external conditions upon the germ-plasm is, in fact, freely admitted. After speaking of the effects of warmth and cold on the pupæ of certain butterflies, Weismann says (II, p. 273): "It is thus intelligible that somatic variations like the blackening of the wings through warmth appear to be directly inherited and accumulate in the course of generations; in reality, however, it is not the somatic change itself which is transmitted,

[1] "Animals and Plants," I, page 7.
[2] "The Evolution Theory," II, p. 266.
[3] "Amphimixis as the means by which a continued new combination of variations is effected," need not be considered here, since this, as Weismann says, is not "the real root of variation" (II, p. 194).

but *the corresponding variation evoked by the same external influence in the relevant determinants of the germ-plasm* within the germ-cells, in other words, in the determinants of the following generation." Elsewhere, also, he had previously said[1]:—"Many climatic variations may be due wholly or in part, to the simultaneous variation of corresponding determinants in some part of the soma and in the germ-plasm of the reproductive cells." This double simultaneous action seems first to have been suggested by Francis Galton in 1895, though it was more definitely expressed later by Cope, who gave to the process the name of 'Diplogenesis.'[2]

Here we have surely the plainest admission from Weismann himself of the very occurrence of the matter in dispute, that is, the transmission of acquired variations. The thing itself is admitted to occur, and it is a matter of altogether minor importance whose hypotheses are capable of giving the best explanation of the mode in which it is brought about. Formerly to satisfy Weismann the change was regarded as primary in the germ-plasm, and owing to nutritional variations therein. Thus, speaking of one of Hoffmann's experiments he says ("Essays," I, p. 426):—"The body of the plant—the soma—has not been directly affected by external influences, in Hoffmann's experiments, but changes have been wrought in the germ-plasm of the germ-cells and, only after this, in the *soma* of succeeding generations . . . the idioplasm was changed more and more in the course of generations, until at last the change became great enough to produce a visible character in the *soma* developed from it. Because in this, and in many other cases a particular change did not "become visible until after some generations had elapsed," Weismann here assumes that the change must have been initiated in the germ-plasm itself.[3] But whether that is true, and whether production of the new characters in plant or animal must of necessity be *initiated* in the germ-plasm, anterior

[1] "The Germ Plasm," Contempy. Sc. Series, 1893, p. 406; see also "Evolution," II., p. 272.

[2] "American Naturalist," Dec., 1889, published in 1890.

[3] On the other possibility some hints are given by Herbert Spencer in "Biology," II., p. 620. Light is thrown upon such a process also by certain experiments with the larvæ of butterflies. Referring to them Cope (*loc. cit.* p. 440) says:—"In another experiment, larvæ which were in the act of weaving cocoons, on exposure to certain colours, were induced to weave cocoons of corresponding colours. This experiment demonstrates that a stimulus may be transmitted to a gland so as to modify the character of its secretion in a new direction. . . . This prepares us to look upon heredity as an allied phenomenon, *i.e.*, the transmission of a special energy from a point of stimulus to the germ-cells."

to correlated changes in the soma, rather than *induced* in the germ-plasm simultaneously as a result of the operation of the same causes which produce the definite changes in the soma, is a question perhaps of some theoretical interest (though practically insoluble): but it is not the question with which we are immediately concerned[1] and is of little or no importance since Weismann himself now admits the diplogenetic process.

Irrespective then of the mode in which it is brought about, we see that Weismann frankly admits the whole point in dispute—namely, that *acquired characters can be, and are, frequently inherited.* Thus speaking of another experiment of Hoffmann's he says:—"He succeeded in inducing considerable changes in the structure of the root of the wild carrot (*Daucus carrota*) by means of the changes in nutrition implied by garden cultivation. These changes also proved to be hereditary." References to many other striking changes of this kind will be found in an article on 'Recent Science' by Prince Kropotkin.[2] Thus, he says, Vilmorin obtained the cultivated carrot out of the wild one in five generations. Carrière did the same with the radish, and Buckman the same with parsley. Multitudes of experiments have been recorded showing that a direct action of external conditions will even produce 'adaptive' changes in plants, so that, as the above-mentioned writer says, "when we see that environment so rapidly itself creates the adaptation, we shall necessarily be more cautious in speaking of the natural selection of quite accidental individual variations."

It seems only natural to suppose that changes wrought by external conditions should require four or five generations to attain their maximum, whether such changes take effect chiefly upon the soma or upon the germ-plasm; and what more natural than to suppose when a plant has been thus altered under a given set of

[1] In one of the earlier pages (p. 403) of this same "Essay," he had said:—"But obviously it is of no importance for the question of the transmission of acquired characters, whether the changes directly produced by external influences upon the *soma* of an individual are greater or smaller: *the only question is whether they can be transmitted.* If they can be transmitted, the smallest changes might be increased by summation in the course of generations, into characters of the highest degree of importance. It is in this way that Lamarck and Darwin have supposed that an organism is transformed by external influences." But from the point of view of "*the only question*" in dispute, it is quite immaterial whether the changes begin in the soma, or in the germ-plasm, or in both concurrently.

[2] "Nineteenth Century and After," 1901, Vol. 50, pp. 424–438.

new conditions, that the replacing of it in its old habitat should cause it to go through the reverse set of changes, leading to a resumption of its original form and characters.[1]

The plasticity of plants has long since been shown to be most remarkable, alike from the extent and the rapidity of the variations that have been induced. Thus in a previous article[2] Prince Kropotkin cited numerous experiments showing remarkable alterations induced in plants by changes in their environment: transferring them, for instance, from the lowlands to mountain elevations; subjecting others to a deficiency of light; or exposing them to a very dry air and so producing prickles by gradually diminishing the size and development of the leaves. In this latter case the growth of plants in different degrees of moisture of itself seemed to produce the structures best adapted for avoiding excessive evaporation. So that, as was said by Kropotkin, "several adaptations which were considered as slowly accumulated accidental variations, can be obtained very rapidly—as a direct result of environment itself." Vernon, indeed, in his work on "Variation in Animals and Plants,"[3] feels warranted in saying that "adaptability would seem to be a fundamental property of protoplasm"; and in the pages indicated he cites numerous cases in illustration of this view from experiments of different kinds made with plants. It is true that in some of these cases the question arises whether the indirect influence of external conditions, that is 'natural selection,' may not also have been operative (*loc. cit.* p. 391).

Although the influence of change in the environment does not produce such rapid and marked effects upon animals as it does upon plants, it is none the less a cause of change in them. Differences in quantity or quality of food, or of both combined, are among the most potent of these slowly acting causes. Thus Prof. Brewer of Yale University says: "Breeders do not believe that the characters acquired through the feeding of a single ancestor, or generation of ancestors, can oppose more than a slight resistance to that force of heredity which has been accumulated through many preceding generations, and is concentrated from many lines of ancestry. Yet the belief is universal that the acquired characters due to food during the growing period has *some* force, and that

[1] Such phenomena are thoroughly well known and recognised in regard to Bacteria; while L. Errera, Klebs and others have shown that similar phenomena are to be met with among Fungi.

[2] "Nineteenth Century," April, 1894, pp. 688–691.

[3] Internl. Sc. Series, 1903, pp. 373–378.

this force is cumulative in successive generations. All the observed facts in the experience with herds and flocks point in this direction."[1]

Apart, however, from the question of adaptability we must not lose sight of the remarkable changes recorded in the last chapter in illustration of 'mutation.' These changes would, of course, be regarded by Weismann as due to what he calls 'spontaneous' changes in the germ-plasm. But no distinct boundary-line can be drawn between cases of 'induced' changes in the germ-plasm and those which he speaks of as being 'spontaneous.' Has he not himself said that *all* such changes "have their roots in external influences"? And in the case of the tall maize, in the various new species of the Evening Primrose, as well as in that of the black-shouldered Peacock, we have notable instances of acquired characters being transmitted to succeeding generations.

It is, however, changes strictly belonging to Weismann's category of 'induced' germinal selection which will be found to afford the most weighty evidence for the solution of our problem, and in regard to these cases the statements made by Weismann are most emphatic and precise. He says, for instance, "It is indubitable that external influences, such as those emanating from the environment or media in which species live, are able to cause direct variation in the germ-plasm, that is, permanent, because hereditary variations. We have already referred to this process and called it 'induced' germinal selection" (II, p. 267). While again he speaks of the germ-plasm as "ready and able to furnish any variation that is possible in a species, if that is required by external circumstances" (II, p. 196).

Here the real point in dispute seems to be frankly admitted. And the fact that acquired characters can be inherited is the real point of importance: it is, as I have said, altogether a matter of subordinate interest whether the change in external conditions acts first upon the soma, or first upon the germ-plasm, or simultaneously upon both.

We have, however, other evidence bearing upon this latter point, which must not be lost sight of. Thus, that heritable variations may be created in, and transmitted by, the soma only seems to be shown by the phenomena of pelorism, to which reference was made in the last chapter (p. 106), and of bud-

[1] Quoted by Cope in "The Primary Factors of Organic Evolution," p. 424.

variation generally. These, or at all events the majority of them, appear to be local effects not dating from the germ-plasm at all, but from the soma, and that often at a comparatively late stage of its development. The occurrence of such mere local changes was fully recognised by Spencer as not necessarily taking their origin in the germ-plasm, since when speaking of his physiological units he said (I, p. 369) they must be presumed to have "such natures that while a minute modification, representing some small change of local structure, is inoperative on the proclivities of the units throughout the rest of the system, it becomes operative in the units which fall into the locality where that change occurs." And in regard to what is known as "irregular peloria" Maxwell Masters[1] makes the definite statement that this form of "peloria is evidently not congenital, but occurs at a more or less advanced stage of development."

Bud-variations generally are now known to be excessively numerous. As far back as 1895, according to Prof. Bailey of Cornell University, no less than 300 of such bud-sports were known and actually under cultivation in the United States. And as showing that these bud-variations and germinal-variations are essentially similar in kind, though the change in the one case takes place in the soma and, in the other, in the germ-plasm, there is the fact, as Lloyd Andriezen points out,[2] that "selection can be practised for the improvement and definiteness of forms originating by either means."

There seems, therefore, absolutely no room for doubt that local variations may take their origin in the soma and may be perpetuated, selected and increased therein, in the form of bud-variations. It seems equally clear that in other cases we may have actual new species of plants arising by 'mutation,' as a result of *quasi*-'spontaneous' changes, either in the germ-plasm or in the soma; and that in other cases, still under the influence of altered external conditions—whether acting primarily upon the soma or upon the germ-plasm or concurrently—we may find variations arising in plants and animals which are capable of being inherited and transmitted to their progeny. The evidence, as we have seen, is equally clear also in favour of the heritable effects of use and disuse.

There is thus reason to believe that we have other most impor-

[1] "Vegetable Teratology," p. 229.

[2] "The Problem of Heredity, &c." "Jrnl. of Mental Science," January, 1905, p. 20.

tant factors for ever co-operating with Organic Polarity and Natural Selection as causes of Evolution, and as producers and maintainers of new organic forms. And if such causes have ever been in operation the rate of Evolution must have been very notably greater than that which would have resulted from the sole action of a process like Natural Selection, which, from its very nature, is one that must of necessity bring about its changes slowly and gradually, because of their need of development through many generations. As Darwin said, "it can act only by very short and slow steps." For such a process alone to achieve what has been brought about in the way of organic Evolution would, as I have already intimated, require prodigious periods of time far exceeding anything that geologists would demand, or which physicists are prepared to grant.

Yet we find Weismann an implicit believer in this one cause. He holds, as we have seen, that "there is no such thing as a 'tendency' of the protoplasm to vary," and its probably varied isomeric changes are never once referred to. He appeals to chance variations in the nutrition of his determinants, due to irregularities in the food supply that reaches them within the microscopic granules of which the chromosomes are composed, and regards 'natural selection' as an all-sufficient cause of organic evolution. I, on the other hand, am impressed with the intrinsic mutability of living matter and its innumerable isomeric states; and see in this fundamental property of protoplasm a quality which would cause it to vary in form under the influence of its own changing states and 'molecular polarities'; which would make it sensitive to all external influences, whether acting directly or indirectly; which would enable it to respond to the effects of use and disuse, and make it prone from slight, often imperceptible, causes to undergo *quasi*-spontaneous isomeric variations, leading, according as the organism is low or high in the scale of being, either to actual heterogentic transformations or to comparatively slight specific 'mutational' variations. The conjoint co-operation of causes and influences of this kind, acting, not only now, but during all geologic ages, would make it no longer necessary for biologists to require that so many hundreds or even thousands of millions of years should have passed since living things first appeared on the surface of the earth.

CHAPTER VIII

CONCERNING THE PRESENT OCCURRENCE OF ARCHEBIOSIS

EVOLUTION implies continuity and uniformity. It teaches us to look upon events of all kinds as the products of continuously operating causes—it recognises no sudden breaks or causeless stoppages in the sequence of natural phenomena. It equally implies that natural events do not vary spontaneously. It is a philosophy which deals with natural phenomena in their widest sense: it embraces both the present and the far-distant past. It assures us that the properties and tendencies now manifest in our surrounding world of things are in all respects similar to those which have existed in the past. Without a basis of this kind, the Evolution Hypothesis would be a mere idle dream. Uniformity is for it an all-pervading necessity.

Starting from facts of daily observation and from scientific experiments, the properties and tendencies of things have been noted and grouped; whilst philosophers, using the knowledge thus gained, have sought to trace back the progress of events and show how this complex world has gradually been derived from a world of more and more simple composition. We are taken back in imagination even much further. We are referred to a primal haze or nebula—as the gigantic germ of our Solar System. This was the conception of Kant and of Laplace.

But whether we follow the philosopher in his bold speculations concerning the past, or listen to the biologist making his predictions as to the future stages which the germ of a given animal will pass through in the progress of its development—in each case the 'uniformity of nature' is tacitly assumed. This assumption underlies almost all our thoughts and actions, even in every-day life. And without such a belief, regarding the succession of events, science would be impossible—the very idea of it, in fact, could

never have arisen. In the absence of 'uniformity' we could neither fathom the past nor illumine the future. As John Stuart Mill said[1]: "Were we to suppose (what is perfectly possible to imagine) that the present order of the universe were brought to an end, and that a chaos succeeded in which there was no fixed succession of events, and the past gave no assurance of the future, if a human being were miraculously kept alive to witness this change, he surely would soon cease to believe in any uniformity, the uniformity itself no longer existing."

It is true that in earlier times no absolute belief in the uniformity of nature existed, even among the select few. The Greek philosophers, including Aristotle, recognised 'chance' and 'spontaneity' as finding a definite place in Nature, and to this extent they were not sure that the future would resemble the past. But as we have become more familiar with a wider range of natural phenomena and with their mutual relations or order of appearance, so has the conception of chance or spontaneity disappeared from the scientific horizon—driven out of the field by the steady advance of Law and Order. Those who embrace the Evolution Philosophy are foremost in this opinion—they believe that no effects, of whatsoever kind, can occur without adequate causes; and the conditions being similar, that similar results will always follow the action of any given cause. Their whole creed is, in fact, pre-eminently based upon an assumed Uniformity of Nature.

The properties and chemical tendencies of material bodies have, in fact, been found to be quite constant through time and space. Speaking upon this subject in a celebrated discourse on 'Molecules,' Prof. Clark Maxwell said,[2] "We can procure specimens of oxygen from very different sources, from the air, from water, from rocks of every geological epoch. The history of these specimens has been very different, and if, during thousands of years, difference of circumstances could produce difference of properties these specimens of oxygen would show it. . . . In like manner we may procure hydrogen from water, from coal, or as Graham did, from meteoric iron. Take two litres of any specimen of hydrogen, it will combine with exactly one litre of any specimen of oxygen, and will form exactly two litres of the vapour of water. . . . Now if during the whole previous history of either specimen, whether imprisoned in the rocks, flowing in the sea, or careering through

[1] "System of Logic," 6th Edn., Vol. II, p., 98.
[2] "Nature," Sep. 25, 1873, p. 440.

unknown regions with the meteorites, any modification of the molecules had taken place, these relations would no longer be preserved. . . . But we have another, and an entirely different method of comparing the properties of molecules. The molecule, though indestructible, is not a hard rigid body, but is capable of internal movements, and when these are excited it emits rays, the wave-length of which is a measure of the time of vibration of the molecule. . . . By means of the spectroscope the wave-lengths of different kinds of light may be compared to within one ten-thousandth part. In this way it has been ascertained, not only that molecules taken from every specimen of hydrogen in our laboratories, have the same set of periods of vibration, but that light having the same set of periods of vibration, is emitted from the sun and from the fixed stars. . . . We are thus assured that molecules of the same nature as those of our hydrogen exist in these distant regions, or at least did exist when the light by which we see them was emitted."

With evidence such as this before us, which could easily be multiplied to an enormous extent, we should hesitate before needlessly postulating any infringement of the uniformity of natural phenomena. But to assume, as the great majority of Evolutionists do that Archebiosis, or the natural origin of living matter, took place once only in the remote past and that it has not been repeated, or if repeated in past times, that it no longer goes on, is to look upon this process as a kind of natural miracle, and to postulate a break in continuity which ought only to be possible in the face of overwhelming evidence of its reality. This latter is, however, as I contend, altogether absent to anything like an adequate extent.

For living matter to have come into being in the remote geological past, when nothing of the kind had previously existed, must have been a far more difficult thing than for it to arise *de novo* now, and during past ages, after living things had been plentiful on the face of the earth. Originally there would have been no organic compounds in the waters, diffused from pre-existing living things, such as commonly exist at the present day—matter of this sort would have had itself to come into being by natural agencies before the next step in complication could occur : that is, the formation of living matter itself.

Two of the most consistent Evolutionists from this point of view have been Haeckel as zoologist, and Carl Nägeli as botanist. Both of them, but especially the latter, believe that Archebiosis is a

process that has been many times repeated. In regard to the original process itself and our ignorance of the steps by which it occurred Nägeli says :—" The origin of the organic from the inorganic is, in the first place, not a question of experience and experiment, but a fact deduced from the law of the constancy of matter and force. If all things in the material world are causally related, if all phenomena proceed on natural principles, organisms, which are formed of and decay into the same matter, must have been derived originally from inorganic compounds."

What the actual steps of the process were in the past, or what they are now, if living matter is still being formed from its elements, is absolutely unknown. Pflüger in 1875 put forward the notion that cyanogen and its compounds may have been formed when the earth was in a state of incandescence or great heat, and that changes of this kind may have formed the starting point from which much more complex molecules were ultimately produced. The cyanogen-radicle is in fact believed by him to be always present in protoplasm and to be one of the things which mainly helps to confer its characteristic properties upon protoplasm. Pflüger, indeed, says that cyanic acid might almost be described "as a semi-living molecule," owing to its tendency to grow by concatenation of atoms.

But there is another possibility—perhaps even simpler—that must not be lost sight of. We have seen in Chapter III that living matter grows freely, that it is capable of building up its own substance from a simple solution of ammonic tartrate in distilled water and still more freely when a little phosphate of soda has been added to the solution. Nitrate of ammonia, moreover, is known to be produced, in the air during thunderstorms. It is formed, as Dumas says, "upon a grand scale by the action of those magnificent electric sparks which dart from the storm-cloud, and furrowing vast fields of air, engender in their course the nitrate of ammonia which analysis discovers in the thunder shower." And this nitrate of ammonia, so formed, together with water, carbonic acid and a few other simple saline ingredients are, as is well known, the materials which plants of all kinds use for the building up of their own substance.

When we find, therefore, that Bacteria and Torulæ can grow freely in a simple solution of ammonic tartrate and sodic phosphate, the view seems forced upon us that the synthesis of living matter may be, in its lowest phasis, a much simpler process than has hitherto been imagined. We find Bacteria, which have been pre-

viously living a totally different life in organic fluids, when introduced into this simple solution which may be almost described as an inorganic mixture,[1] at once altering their life processes in a fundamental manner, and forming their protoplasm in some mysterious way from the mere elements of the saline solution. It would seem, therefore, that the process in this case cannot be a very complicated one; and when we think of the intimate relations existing between Origination and Growth in simpler forms of matter, and the examples that have been given in a previous chapter (pp. 47, 60), we may be encouraged to hope that some conditions may be at last discovered, under the influence of which it may be freely admitted that living matter will take its origin, as well as merely grow, within some simple saline solution.

Let us look at the question of origin and growth for a moment in its simpler form—taking the case of the production of a so-called "silver tree." A weak galvanic current is passed through a solution of silver nitrate, and simultaneously in a first increment of time a number of molecules of oxygen and of silver begin to aggregate independently into crystals of silver oxide; in a second increment of time, the operation of the same causes produces similar results, only now part of the new crystalline matter forms in connection with the existing recently-formed germs of crystals, though part of it may still aggregate independently. During a third, a fourth, and all succeeding increments of time in which the same causes operate amidst similar conditions, similar results must ensue. But taking the process of origination that occurs in the first increment of time, can we believe that it is in any essential way different from that process of growth which takes place during a second, third, or fourth increment of time? Does not the very fact that origination and growth so often occur simultaneously in the case of crystalline matter, and under the influence of the same causes, show us that the two processes are intrinsically similar, and that conditions favourable for growth are also likely to be favourable for origination? And if this be true for crystalline matter, may we not infer that it should also be true for living matter? These are questions usually neither asked nor answered in any definite manner. They are, however, by no means unworthy of an attentive consideration.

[1] Ammonium tartrate can now be produced synthetically by chemists in the laboratory, from inorganic material, and the substance so produced is, Sir William Ramsay tells me, "so far as we know, absolutely identical with ammonium tartrate derived from tartaric acid abstracted from wine-lees."

When Bacteria are introduced into the ammonic tartrate solution unfortunately we have no means of knowing whether origination, as well as growth and multiplication, occurs. We do not know for certain whether the influence of a pre-existing nucleus is relatively more potent or more necessary in the case of living matter than it is in the case of crystalline matter, but it ought not to be assumed to be so except upon evidence of a clear and unambiguous nature.

The probabilities, therefore, would certainly seem to be altogether in favour of the continuance of a natural process like Archebiosis after it had been once initiated, especially when we see how easily the 'growth' of living matter from mere inorganic elements is occurring over almost all parts of the earth's surface. Let us look now, however, to the principal adverse considerations usually brought forward.

Much stress has been laid by certain writers upon the fact that "the doctrine of spontaneous or equivocal generation has been chased successively to lower and lower stations in the world of organised beings as our means of investigation have been improved."[1] So that, as another eminent writer says, "if some apparent exceptions still exist they are of the lowest and simplest forms."[2] And it is usually inferred from this latter fact that further knowledge and improved means of observation will prove these apparent exceptions to be no exceptions to the supposed general rule—*omne vivum ex vivo.* A consideration of this kind seems to have powerfully influenced Prof. Huxley.

But much confusion exists in reference to this point which needs to be removed. The absurd ancient notions on the subject of 'Equivocal or Spontaneous Generation' to the effect that rats were produced from the mud of the Nile, eels from the mud of rivers generally (Aristotle), that bees could be born from the putrefying flesh of an ox (Virgil), and other similar fancies have, of course, no bearing upon the subject at all. Then, again, under this term 'Spontaneous Generation,' it has been the custom to confuse two wholly distinct processes—namely Archebiosis and Heterogenesis —which require to be considered and dealt with altogether apart. We have nothing to say concerning Heterogenesis at present; we are considering Archebiosis only, and in regard to this process no

[1] Prof. (now Lord) Lister, Introductory Lecture (University of Edinburgh), 1869, p. 12.

[2] Mr. Justice Grove (Presidential Address) Rept. of Brit. Assocn. 1866, p. 71.

believer in Evolution could ever suppose that it had to do with the origination of any but "the lowest and simplest" organic forms.

When it is said that a belief in 'Spontaneous Generation' (Archebiosis) would tend to contradict the experience of all mankind, as we are accustomed to see living things invariably proceed or take their origin from other living things, my reply is that Archebiosis may be occurring all round us, and that from its very nature it must be a process lying altogether outside human experience—and never likely to come within the actual ken of man. So that even if it had been given to Prof. Huxley, as he said [1] in a celebrated Presidential Address, to "look beyond the abyss of geologically recorded time" it would have been excessively unlikely that he would have been enabled to witness, as he intimated, an "Evolution of living protoplasm from not-living matter." At the most he might have seen then (but only if equipped with a powerful microscope) just what he might have seen in his own day under more favourable conditions, namely, a gradual emergence into the sphere of the visible, in some suitable fluid, of the minutest specks of living protoplasm.

It is scarcely conceivable for an Evolutionist to suppose that living matter could ever take origin, either now or in the past, in any other way. So that the birth of ultra-microscopical particles gradually coming into the region of the visible may easily be understood as a process, in its incipient stages, lying altogether outside the range of man's experience, and as one which, even in its later stages, has been seen only by a few who have specially looked for it.

In Chap. III (pp. 50–54) I have told all that can be *seen* of what appears to be the origin of particles of living matter—the possible process of Archebiosis—and have made known an easy means by which others may, if they proceed with care, observe for themselves this gradual emergence of living particles from the region of the invisible. They do not proceed from any germs which are known to science. Science knows only visible germs. Moreover, I have shown that the very minute particles which appear, speedily grow into Bacteria or into Torulæ of one or other kind; and that these forms of life are just as plastic and modifiable as we should expect them to be if they had in fact been derived from new-born particles of living matter—this being a substance which, as we have seen, by reason of its extreme molecular complexity, has a high intrinsic tendency to variation.

[1] Report of the Brit. Assocn. for the Advancement of Science, 1870.

Everything, therefore, tends to support the doctrine of Continuity, and seems to testify to the present occurrence of Archebiosis—especially when the alternative we are asked to accept is the belief that these particles which we see gradually emerging into the region of the visible, and developing into simplest organisms whose form and mode of growth varies with every slight change in the media in which they appear, have been perpetuating themselves in this humble form through hundreds of millions of years, while other portions of the same variable living matter have gradually given rise to all the higher forms of life that have ever appeared upon the earth.

It is, of course, not to be supposed that I regard Bacteria and Torulæ as the only primordial forms of life. It is possible that the simplest Amœbæ, and the simplest forms of Algæ—especially the non-nucleated Chromacea—may also be primordial living things. In all mountainous countries, and wherever large surfaces of rock are bared, we may constantly see great black patches, often many yards in diameter, looking at a distance like patches of paint. On close examination we may find that such a patch is composed of a thin, blackish, granular layer most closely adherent to the surface of the stone which is otherwise quite bare. Portions of such a limestone rock, thus coated, have been brought home by me, and microscopical examination of some scrapings from the black patch, which was soaked in water for a short time, has shown it to consist of an aggregation of minute purple algoid vesicles, some extremely minute and others about $\frac{1}{4000}''$ in diameter mixed with a smaller number having an orange or red-brown tint. They were all, however, simple Algoid cytodes presenting no trace of a nucleus—nor were filaments of any kind to be seen among the vesicles.

We have taken the question of the present occurrence of Archebiosis as far as it can be taken, from the point of view of logic, of continuity, and of actual observation; so that we now have to turn to another aspect of the question, namely to a brief consideration of some of the latest experimental trials that have been made—of attempts actually to prove that a natural origin of living matter is still possible under this or that experimental condition.

On this subject Weismann says ("Evolution," II, p. 366):—"After the fiery earth had so far cooled that its outermost layer had hardened to a firm crust, and after water had condensed to a liquid form, there could at first only have been inorganic substances in existence. In order to prove spontaneous generation, therefore,

it would be necessary to try to find out from what mingling of inorganic combinations organisms could arise; to prove that spontaneous generation could never have been possible is out of the question. . . . It would be impossible to prove by experiment that spontaneous generation could *never* have taken place; because each negative experiment could only prove that life does not arrive *under the conditions of the experiment.* But this by no means excludes the possibility that it might arise under other conditions."

What he says in the last two sentences is, of course, quite true; but what he said previously narrows the conditions of the experiment overmuch. We do not now so much want to prove the problem as it presented itself in the past, when there were only "inorganic substances in existence." However interesting that problem may be, our more immediate interest is to know whether Archebiosis can be proved to take place now, under the easier conditions provided by the plentiful existence of organic matter in solution. Although experiments have been made in both directions, still the trial has for the most part been made with solutions or infusions containing organic matter; and, owing to the doubts and difficulties inseparable from the problem, it must be confessed that the results have not been such as to carry conviction to the minds of men of science generally. Many have been led, in fact, to attach more weight to the supposed non-success of the experimental evidence than there was any real warrant for, and, not mindful of the strict limitations of the scope of such evidence, as indicated above by Weismann, have seemingly allowed their judgments to be unduly prejudiced in regard to the general question. From this point of view the influence in this country, during the last generation, of Huxley, of Tyndall, and of Herbert Spencer, and, in France, of Pasteur has been altogether unduly to decry the very possibility of the occurrence of Archebiosis. Only one philosophic thinker in this country during that period seemed to feel the full weight of the general evidence. That was George Henry Lewes who wrote as follows:[1] "All the complex organisms are evolved from organisms less complex, as these were evolved from simpler forms: the link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*. . . . It is therefore consistent with the hypothesis of Evolution to admit a variety of origins or starting points."

[1] "Fortnightly Review," 1868, I, p. 373, II, p. 79.

The requirements, underlying all attempts to prove the "spontaneous generation" of living matter by flask experiments with superheated fluids, are of two kinds. We have (*a*) to ascertain as far as possible, by preliminary trials, the lowest amount of heat, and the duration of its application, which is necessary for the destruction of pre-existing living things within the experimental vessels. And we have (*b*) to see whether it is possible that fluids submitted to the *lowest necessary amount of heat* to ensure the destruction of all pre-existing life can still, by subsequent treatment under what are at the best only very unfavourable conditions, be induced to engender living matter.

Everything of course turns upon these words "lowest necessary amount of heat."

The destructive and deteriorating influence of heat upon the organic fluids with which experiments have to be made must be obvious to all; and the ultimate deteriorating results may not be very different where recourse is had to one process of heating at a high temperature, or to several successive heatings at rather lower temperatures as in the "discontinuous process" adopted by Tyndall. No one need doubt the truth of the naïve remark of Fischer[1] when he says, "Infusions of organic substances, if they be only boiled long enough, will remain sterile for years."

The essential question is, therefore, what is the lowest necessary amount of heat to ensure, on the one hand, that all pre-existing living things shall be killed and to avoid as far as possible extreme deterioration by heat of the fluids employed. It has now been thoroughly brought home to us how impossible it is to arrive at a trustworthy and generally acceptable conclusion in regard to this question. In death-point experiments (*a*) the barrenness of the fluid may merely mean that the organisms are inert rather than dead. I freely admit this as a possibility, and moreover, judging from the past history of this question, feel certain even if there were a new basis of agreement in regard to the death-point of micro-organisms in any particular fluid, that this common point of agreement would be immediately renounced, by many, directly some experimenter claimed that from such a basis he had demonstrated the 'spontaneous generation' of living matter. He would doubtless be told that 'spontaneous generation' was a chimera, and that he had only succeeded in resuscitating germs which all

[1] "The Structure and Functions of Bacteria," Trans., 1900, p. 51.

his predecessors had hitherto believed to be dead. The proof of this being so would for them (as believers in the chimera assumption) lie in the simple fact that living things had appeared within the experimental vessels. Such see-saw work might go on indefinitely. Conviction would probably never be brought about, because this ever-recurring objection could not possibly be met, unless the experimenter confined himself to sterilising degrees of heat which would allow little chance of success. The last experiments I published on this subject, together with this kind of interpretation of their results given by others, were of such a nature as to convince me of the futility of further attempts in this direction. They tended to show the practical insolubility of the question by flask experiments with superheated fluids, because of the immediate renunciation by opponents of previous beliefs, whenever results would otherwise show that 'spontaneous generation' had been demonstrated. For that reason I ceased to make and publish further experiments of this kind.

Anterior to the publication in 1876 of my experiments on the fertilisation of boiled acid urine by an accurately neutralising dosage with boiled liquor potassæ,[1] it had been generally inferred by all workers on this subject that the continued barrenness of urine and other acid fluids previously superheated was a proof that heat to the extent employed was adequate to kill any organisms or their germs pre-existing in such fluids.

This view was based upon the fact that living Bacteria or their germs seemed invariably to increase and multiply in suitable fluids placed under conditions favourable for fermentation.

It must be admitted, however, that this assumed proof was fallacious. Life may persist even when there is not sufficient vigour in the living things to enable them to grow or multiply in the previously heated medium in which they find themselves.

And as long as such inert or partially devitalised organisms remain in certain fluids, they may never regain sufficient vigour to enable them either to grow or to multiply. They may continue as if dead, though not actually dead.

But in other fluids of a different chemical constitution, or nutritive value, it is certainly possible that such partially devitalised organisms may become reinvigorated (especially if their revival be further aided by more stimulating conditions) so as to enable them

[1] "Proceed. of Royal Soc.," 1876, vol. xxv., p. 149, and "Journ. of Linn. Soc." (Zool.), vol. xiv., 1877—these being my last contributions to this side of the question.

again to grow and multiply. In this way it must be admitted as possible that life may appear in previously superheated fluids simply because organisms, hitherto assumed to be dead, have been made by the means adopted to show themselves as living.

Still the belief that the barrenness of a superheated fluid (favourable in nature and favourably placed for the promotion of fermentation) was due to the fact that all its contained organisms and their germs were killed—had been commonly accepted as true and the results of experiments of such an order had been cited as an important portion of the evidence and arguments tending to show, as Pasteur declared, that "spontaneous generation" was a chimera. My successful experiments with boiled acid urine subsequently neutralised by liquor potassæ, at once seemed to necessitate the rejection of this belief by those who had previously proclaimed it. I was, in effect, promptly told that the organisms which all previous experimenters had spoken of as being killed in boiled acid urine could not have been killed. I had by subsequent neutralisation of the fluid, and by exposing it to a far higher incubating temperature (50° C.) than had been employed by previous experimenters, succeeded, they in effect said, in bringing to life organisms which every one else had supposed to have been killed. It was this, rather than Archebiosis, which accounted for the swarms of Bacteria that speedily appeared within the experimental flasks.

Subsequently we find, from a thesis published in 1879,[1] that Chamberland, a previous assistant of Pasteur, tests the vital resistance of Bacilli spores by heating them in *neutral* infusions, which may be capable of *originating* as well as of favouring the growth and multiplication of such bodies, and he concludes that a heating for five hours at 100° C. is needful for their destruction in some of these fluids.

But many years ago, after what I considered to be some very extravagent statements by Tyndall concerning the powers of resisting heat possessed by, what were for him, hypothetical germs contained in "old hay," I made a long series of experiments with multitudes of actual hay germs (that is, with spores of the hay Bacillus) which had been allowed to dry in their mother liquid on glass slips five years previously. I tested the death-point of these old Bacilli spores in acid urine that had been boiled, in

[1] "Recherches sur l'origine et le développement des organismes microscopiques." *Thèse*, No. 420. Faculté des Sciences, Paris.)

which I knew that Bacilli or any such organisms did not originate; though I had ascertained that the inoculation of boiled urine with these spores when they had not been previously heated would cause Bacilli to grow and multiply therein most freely. An emulsion of the old spores was made by scraping the dried scum from the glass slips into a small quantity of water, followed by a vigorous shaking. Drops of this emulsion were then added to some acid urine, and this inoculated fluid was subsequently boiled for twenty minutes, with the result that the spores invariably *seemed* to be killed—seeing that the fluids remained unchanged.

Thus, boiling for twenty minutes in a slightly acid fluid seems to destroy the actual spores, while boiling for five hours is said to be necessary in another neutral fluid (hay infusion) to ensure the destruction of similar spores possibly present. This, of course, may be true, or it may not. Spores actually existing in the hay infusion may be much more speedily killed; and it may be the power of giving birth to organisms (the germinality of the fluid) which requires the five hours at 100° C. for its extinction.

It is true that there is good evidence (such as Chamberland advances, and of a kind to which I had long ago called attention) to show that growth and multiplication of Bacilli does not take place so freely or quickly in slightly acid as in slightly neutral solutions. But then might not, even ought not, the same difference to hold good in regard to the possible origin of living units in the respective fluids? The conditions less favourable for growth should be less favourable for origin, and *vice versâ.*

The difference between twenty minutes and five hours of exposure to 100° C. is enormous; but the difference between the rate of growth and multiplication of Bacilli in slightly acid and in slightly neutral fluids respectively, however real, is comparatively insignificant.

Hence the question of the possible germinality of the hay infusion cannot be ignored. But here again, as ever, we cannot completely get rid of the doubt, that in my death-point experiments the spores were not really killed by the twenty minutes' boiling in the acid urine. Chamberland might retort that they may have been merely rendered inert by the heat so long as they remained in this fluid. To attempt to get rid of this difficulty is to come face to face with its correlative, namely, how are we to decide, when dealing with other more favourable fluids, whether an

appearance of organisms therein is to be ascribed to *survival of germs*, or to *germinality of the fluids*.

The tendency with Pasteur, and with Tyndall and others has been too much altogether to ignore this latter possibility, to regard it as a chimera, and one not to be seriously considered. But this, as was well said by Rücker in his Presidential Address before the British Association in 1901, concerning the one-sided attitude of some in reference to a fundamental physical problem, is "*to beg the whole question at issue; to decide the cause before it has been heard.*"

It is worthy of note, moreover, that my opponents in this question have invariably assumed, rather than proved, various points needful to give adequate warranty to their interpretation of the appearance of living organisms in critical flask experiments. They know quite well that spores are distinctly more resistant to heat than the parent organisms; they know that the parent organisms without spores are almost infinitely more common than such organisms with spores; they know that even under most favourable conditions such spores will often refuse to develop; yet whenever living organisms appear in the guarded fluids which have been heated far more than is necessary for the destruction of the parent organisms, they invariably assume that the much rarer spores have been present, and they further find it necessary to assume that the spores which are often most slow to develop under favourable conditions, now, in spite of the injurious heating and unfavourable conditions in which they are placed, straightway hasten to develop, grow and multiply. This, is surely, more like begging the question at issue, than judging in accordance with evidence.

In addition to the above considerations, it may not be without interest to some that attention should be called to the following additional facts, seeing that they may help to show in which direction the balance of evidence at present lies.

(*a*) *Thermal death-points and their relation to certain old flask experiments.*

The dicta at present given in regard to thermal death-points are these. Pasteur formerly said that a brief exposure to 110° C. was sufficient to sterilise all fluids; but now his former assistant, Chamberland, as a result of investigations made between 1877

and 1879, and recorded in the thesis above referred to, has been induced to put the temperature rather higher. He says, however, that a temperature of 115° C. *is sufficient to sterilise all fluids "completely and very rapidly,"* and therefore to kill any Bacilli spores that may be contained therein.

No statements as to the lowest temperature at which we may be certain that the spores of Bacilli are killed are made by Fischer; he says,[1] however, that superheated steam in an autoclave "kills the toughest spores in one minute at 140° C." Macfadyen also says,[2] "No spore, however resistant, remains alive after one minute's exposure to 140° C."

In reference to the spores of Fungi, de Barry[3] mentions that in their dry state 130° C. has been necessary for their destruction, but he adds "the death-point of the spores of Fungi is often much lower than this *in water or watery vapour, and it has not been shown that any can under these circumstances survive a temperature of* 100 *C.*"

I have, however, recorded experiments in which living organisms have been plentifully found in fluids that had been heated within closed vessels to higher points than any of those above mentioned as lethal.

Thus, experiments were made with neutral and faintly alkaline *hay* infusions of this nature.[4] "In each case about half an ounce of the fluid was used, half filling a tube which was sealed when cold; so that above the fluid there was ordinary air. . . . In twenty-one cases they were heated to 248° F. (120° C.) for thirty minutes; and in five of these latter trials (all with the same hay infusion) no fermentation subsequently occurred. In the other instances more or less distinct fermentation supervened—though in some the signs of change before opening the vessels were only slight.[5] . . . Also in five other experiments in which *milk* was heated to 240° F. (115·5° C.) for ten minutes, fermentation more or less marked occurred in each case in from two to ten days."

The nature of the changes seen in the fluids and the kinds

[1] "The Structure and Functions of Bacteria," Transln., 1900, p. 76.

[2] "Nature," February 7, 1901, p. 361.

[3] "Fungi, Mycetozoa and Bacteria," Transln., p. 347.

[4] See a Memoir "On the Conditions favouring Fermentation," "Journ. of Linn. Soc." (Zool.), vol. xiv., pp. 50–53.

[5] These tubes on account of their subsequent treatment were necessarily of very thick glass, and were therefore thoroughly *flambés* when I prepared them shortly before they were made use of.

of organisms found therein are fully described in this same memoir in subsequent pages (pp. 53–60), and illustrations of some of the organisms met with are given.

Two points worthy of note concerning the changes occurring in such hay infusions are these. When an infusion of this kind has been heated only to 100° C., and subsequently becomes fertile, it may always be seen to change uniformly throughout its whole bulk. There is no cloudiness gradually spreading from centres of infection; but a uniform opalescence, gradually increasing to actual turbidity, throughout the fluid, which at the same time becomes gradually paler in colour. On the other hand, where this or any other clear infusion has been heated to 110° C. and upwards, such a change almost never occurs. We have then altogether different appearances. The bulk of the fluid remains clear, though, after days or weeks, a very slow accumulation of sedimentary matter occurs; and in this, on microscopical examination, in addition to different kinds of Bacteria it is most common to find Torulæ and other Fungus germs together with some Mycelia, such as I have figured as having been found in the experiments above referred to.

The finding, however, of Torulæ and other Fungus germs, with Mycelia, in these superheated fluids, is a fact of great significance in view of the statement by de Barry (with whom others agree) in regard to spores of Fungi generally—namely, that they have never been shown to be capable of surviving in fluids at a temperature of 100° C.[1]

This, however, is not all; I can point to two experiments in which other fluids were heated in closed tubes to 132–135° C. for twenty minutes, and to one in which the fluid was heated to 144° C. for five minutes, and yet after some weeks, when the flasks were opened Torulæ and other Fungus spores were found among the sedimentary matter, such as I have elsewhere described and figured.[2] I can with the utmost confidence refer to these three experiments as being free from all flaw or ambiguity. The

[1] In his "Nouvelles expériences sur la Génération Spontanée" 1864, pp. 126 and 190, Pouchet records experiments in which beer-wort which had been boiling for five and six hours respectively, gave rise in full and hermetically sealed vessels to swarms of Torulæ, in the course of a few days.

[2] See "The Beginnings of Life," vol. I., pp. 441, 443 and 447. During the process of raising the temperature to 132–135° C. and subsequent cooling, these fluids would have been exposed to at least 115° C. for nearly one hour, and of course longer still in the case of the fluid heated to 144° C.

fluids were heated for the times and degrees mentioned in an autoclave, and when the flasks were opened very various living organisms were found in the fluids. Other experiments might be cited, but those referred to are sufficiently typical.

(*b*) *The production of different micro-organisms at will from healthy urine.*

One of the arguments of Pasteur, and also of Lister, William Roberts and others, against the present occurrence of Archebiosis was this. They met the objection that in flask experiments with suitable fluids negative results are due to the degradation of the organic constituents in the fluids, owing to the heating of them many times to minor degrees or once to some major amount, by saying that milk, blood, or urine if taken directly from the body, with all necessary precautions against external contamination, can be preserved indefinitely without change and without the appearance therein of any micro-organism. This of course is what ought to occur if such fluids are, as they maintained, germless, and if the present occurrence of Archebiosis is such a chimera as they imagined it to be.

Twenty years ago I resolved to test these statements for myself, taking urine as the test fluid because of the greater facility for multiplication of experiments therewith. During a period of three months I made nearly two hundred experiments, using all the precautions recommended by Lister as needful in such experiments. The urine was passed either into one of his receivers or else directly into the experimental vessels, and in all cases the vessels employed were thoroughly *flambés*. The urine experimented with had only a moderate amount of acidity, as it was always found to be capable of being neutralised by from 7–10 minims of liquor potassæ to the ounce.

The results that I obtained in the large majority of cases were divisible into four categories :—

(1) Urine passed into a *flambé* vessel, and subsequently kept at temperatures ranging between 16° and 30° C. would, as Pasteur, Lister and others say, remain clear and free from all signs of change.

(2) Urine passed into a *flambé* vessel and subsequently kept at a temperature of about 45° C. would almost invariably become turbid within three days, and on examination be found to swarm with Micrococci.

(3) Urine passed into a *flambé* vessel which had been fully or two-thirds neutralised with liquor potassæ, and subsequently exposed to temperatures ranging from 45° to 50° C. would generally become turbid in seventeen to twenty-four hours, and on examination be found to swarm with Bacilli only.

(4) The addition of lesser amounts of liquor potassæ, that is, sufficient for half neutralisation, or a little less or more, would generally lead to a mixture of Micrococci, Staphylococci, and Bacilli.

I found I could thus obtain, almost at will, urine containing Micrococci only, Bacilli only, or mixtures of these two organisms.

In all the experiments where liquor potassæ was used, I had previously heated it in tubes to 105° to 120°C. for one hour, as I had formerly done to meet objections made by Pasteur in regard to other experiments.

I have little doubt that blood or milk taken from an animal with all the precautions possible, and subjected to such temperatures as I have employed would speedily show signs of change. I have, however, made no experiments with either of these fluids. Were I to do so, and obtain positive results I should doubtless be told that I had not guarded against this or that source of error —which would, in fact, be difficult enough in the case of the latter fluid, with which no one as yet has succeeded in obtaining uniform results.

(*c*) *The changes induced in some organic fluids by their passage through Chamberland and Berkefeld filters.*

Later still, when Berkefeld and Chamberland filters came into use, it was said that the filtration through porcelain, with the latter filter especially, was so effectual that it would stop the passage of all Bacterial germs, and that organic infusions so treated, even when they had undergone no degradation by heat, would remain unaltered and show no signs of fermentation.

If true, this seemed to me at the time the strongest evidence that had yet been brought forward against the present occurrence of Archebiosis, so I resolved to make some experiments myself with hay infusions prepared in the ordinary way. This was done, and I found it quite true that a hay infusion passed through a Chamberland filter would subsequently remain for long periods quite clear, even when kept at a temperature of about 93° F. (34° C.), though I observed that a very slight deposit after a few weeks was apt to accumulate at the bottom of the vessel.

It occurred to me then that the passage through the extremely minute interstices of such a filter might have altogether altered the constitution of the fluid—in a different way, it is true, but with the result of producing changes therein very analogous to those caused by superheating.

I therefore made some experiments in order to test this supposition, and will quote one of the most decisive of them.

On October 20, 1900, a hay infusion was made by macerating two drachms of finely-cut hay in ten ounces of tap water for four hours at about 82° F. (28° C.), and the fluid was subsequently divided into two equal portions.

(*a*) Was simply passed through one layer of Swedish filtering paper.

(*b*) Was passed through a Chamberland filter.

Each fluid was placed in a small lipped beaker to which one drachm of tap water was added; each beaker was then covered with a circle of glass and the two were left side by side on an incubator at a temperature of about 65° F. (18 C.).

In twenty hours the fluid in (*a*) was found very opalescent throughout; that of (*b*) was still clear.

Oct. 23. (*a*) Very turbid, and with a thin pellicle on its surface. (*b*) Still quite clear.

Oct. 25. (*a*) Very turbid, and covered with a pellicle containing Zooglœa areas; no Ciliates seen, but Monads were very abundant. (*b*) Fluid still quite clear.

Oct. 27. (*b*) Fluid still quite clear. It was now inoculated with a small portion of the fluid and pellicle of (*a*).

Oct. 29. (*b*) Fluid still quite clear.

Nov. 1. (*b*) The freely inoculated fluid still remained quite bright and clear at the expiration of five days.

Nothing could show more clearly than this how greatly the hay infusion must have been altered in its constitution by its passage through the Chamberland filter. As I imagined, changes were undoubtedly produced which led to its behaving much as a hay infusion heated to 115° or 120° C. would do. I have often found that such fluids, when removed from experimental vessels, are also very little prone to undergo change, even when freely exposed to air and what it may contain. Moreover, there is another striking resemblance. In the sediment which collects at the bottom of a vessel containing a hay infusion that has been passed through a Chamberland filter, I have, after several weeks, found

Torulæ and other Fungus spores, together with small Mycelia, though the supernatant fluid was still perfectly clear. And, as I have shown (p. 153), the same kind of change is apt to occur in a superheated hay infusion. When either of these fluids is subsequently exposed to the air, a tuft or two of Mould may after a time appear upon its surface; but as a rule, in neither case do the fluids become turbid with Bacteria—at all events within a week or ten days.

Just after I had made these experiments, No. 438 of the "Proceedings of the Royal Society" was received in which I found a communication by Macfadyen, Morris and Rowland "On Expressed Yeast-cell Plasma." They tested the effect of the filtration of this juice through Berkefeld and Chamberland filters, and they found that such "filtration decreases to a considerable extent, but without entirely destroying both the auto-fermentation and the action of the juice on sugar;" they found also that the specific gravity of the filtrate was most notably lowered.[1] Again when speaking of 'kieselguhr,' a very fine diatomaceous earth, which in a compressed state forms the basis of the Berkefeld filter, they say (p. 253): "that kieselguhr has the power of arresting the passage of certain albuminous bodies can easily be demonstrated. Thus we found that egg globulins are almost entirely retained in a kieselguhr sponge, and even albumen and serum proteids are retained to a certain extent."

Thus, removal from an organic solution of some of the larger colloid molecules by filtration, seems to have an effect upon the fluid not very different from that which is produced by the probable breaking up of such molecules during a process of superheating. The fluid in either case is notably degraded, and what I have termed its 'germinality' is proportionately lowered. I am, therefore, no longer surprised at the comparative stability of an unheated hay infusion which has been passed through a porcelain filter.

From this brief indication of the present state of the question in regard to experimental trials, the reader will have gathered something as to the nature of the difficulties besetting the attempt actually to prove the present occurrence of Archebiosis—and how almost hopeless it seems to convince persons, already firmly

[1] Their observations were principally made with a Berkefeld filter, which is known to pass molecules that would be stopped by a Chamberland filter such as I employed.

believing 'spontaneous generation' to be a chimera, that any apparent success is not simply a case of "survival of germs."

Still, some may perhaps be inclined to think with me that the balance of evidence is, even here, distinctly in favour of the occurrence of Archebiosis rather than adverse thereto; although they may also realise that this mode of experimentation with heat-degraded fluids in small vessels cannot in any way be regarded as a fair test of what may now be possible in free nature—that is, with unaltered organic fluids existing under the most favourable conditions.

There is another point also of great importance which has not previously been referred to, that is, the different ultimate questions left in the two modes by which I have endeavoured to throw light on this question. We found (p. 52) that so far as mere *observation* goes, Bacteria may be seen in a thin film of suitable fluid under the microscope gradually emerging from the region of the invisible: the question left here is, have they taken origin from *pre-existing invisible germs*, or have they been formed as ultra-microscopic particles by a synthetic process, followed by growth and visibility. On the other hand, in the *experimental* trials the Bacteria which appear are always assumed to be developed from visible and well-known germs, such as are produced by the hay Bacillus and are known to be able to resist higher degrees of heat than the organisms in which they have been formed: the question that should be left here is really, with most critics, replaced by an *assumption that these known visible germs have been present*, have survived destruction, and have given rise to any new birth of Bacteria that may appear. This assumption, however, covers only part of the ground—not being applicable to Torulæ and Fungus germs generally, since in them no highly resistant 'spores' are known, and de Barry tells us "it has not been shown that any can under these circumstances [that is, in water or watery vapour] survive a temperature of 100° C." The evidence I have adduced, as well as that referred to from Pouchet, is, therefore, altogether in favour of the *de novo* origin of such forms of life.

It thus seems as if we have in reality two distinct and more or less independent methods for attacking the problem as to the present occurrence of Archebiosis—that (*a*) by *experiment* with superheated fluids in closed flasks, and (*b*) another less recognised method, that of mere *observation*, aided by high microscopic powers, of what occurs in thin films of suitable unheated organic

fluids. This latter method calls attention to, and thoroughly exposes the fallacy of, the common belief that the occurrence of 'spontaneous generation' is opposed to the universal experience of mankind. It shows that what is supposed to contradict this common experience lies altogether outside, and, of necessity, completely beyond the range of ordinary human experience. Nobody with unaided eyes could ever have witnessed the birth from fluids of invisible particles, by which the 'spontaneous generation' of living matter must always commence, if it commences at all. And even when aided by the most powerful microscope nobody could decide when *minimum visible* particles appear in the field of view that such particles have proceeded from invisible germs rather than from a primordial synthesis of living units.

But in regard to this latter point I have already shown that absolutely no logical or consistent reason exists for a disbelief in the present occurrence of Archebiosis; and that the general question may be judged quite apart from the fact that, in spite of numerous attempts to prove its occurrence by means of experiment, no one has yet succeeded in convincing the scientific world that a sufficient weight of experimental evidence has been adduced. There is, however, as I have said, no vestige of evidence to show that under favourable conditions the process may not be continually taking place around us. The birth of invisible particles of living matter in suitable fluids in free nature could not oppose or contradict the actual experience of any one. Yet this, and this only, is all that is meant by Archebiosis.

CHAPTER IX

THE HETEROGENETIC ORIGIN OF BACTERIA AND THEIR ALLIES

THE doctrine now to be dealt with, and for the establishment of which I hope to bring forward very conclusive evidence, is one which is as much opposed to common belief as that dealt with in the last chapter. That like produces like—that the offspring of any plant or animal will in all cases develop into an organism like its parent—is a belief as firmly rooted as that other which is embodied in the phrase *omne vivum ex vivo.*

We have found Herbert Spencer saying, "Not only the established truths of Biology, but the established truths of Science in general, *negative the supposition that organisms*, having structures definite enough to identify them as belonging to known genera and species, *can be produced in the absence of germs derived from antecedent organisms of the same genera and species.*" [1] Similarly, Pasteur was no less positive in his opposition to Heterogenesis. "For nearly twenty years," he said, "I have pursued without finding it, a proof of *life existing without an anterior and similar life.* The consequence of such a discovery would be incalculable; natural science in general, and medicine and philosophy in particular, would receive therefrom an impulse which cannot be foreseen." [2]

If I am equally positive, in spite of the dicta of these authorities, that Heterogenesis is a reality, I can only say that this view has been forced upon me after years of work commenced more than thirty years ago, and now again resumed during the last six years.

In Archebiosis we are concerned with the actual origin of living matter, while in Heterogenesis we have to do with the transformation of already existing living matter, and a consequent new birth of alien living things—as when the substance of an encysted Euglena or of an encysted Ciliate becomes transformed into a brood of Monads, Amœbæ, or Peranemata; when similar

[1] "Biology," I, Append. D, p. 168. No italics in original.

[2] "The Life of Pasteur," by Vallery-Radot, 1902, Vol. II, p. 41.

organisms are produced from a Rotifer's egg; or when even Ciliated Infusoria are produced from large encysted Amœbæ or from the great eggs of a Hydatina.[1] These examples will help to bring home to the reader what is meant by Heterogenesis, when I say it is a process in which we have the actual substance of organisms or their germs giving rise to alien forms of life.

The Heterogenetic Origin of Bacteria and their Allies.

In such instances as I have just cited it is easy to see that actual portions of the living matter of the transforming organism or germ are converted, bulk for bulk, into new organisms of a totally different kind. But in most of the cases in which Bacteria and their allies, or Torulæ, are produced in or from the substance of other organisms, whether animal or vegetal, it is practically impossible to say for certain that their origin has been by Heterogenesis rather than by Archebiosis. Such organisms seem always to present themselves first in cells or tissues as extremely *minute motionless particles*,—sometimes where no particles of any kind were previously visible, and sometimes intermixed with particles of a different order, but from which they cannot readily be discriminated. And where the particles of living matter, so individualising themselves, are so small as to be almost beyond the range of our most powerful microscopes, it is impossible to say, until they begin to assume specific shapes, or particular modes of collocation, that they are not normal constituent granules of the tissues in which they are found. But their invariable first appearance as minute, motionless particles, rather than as active, developed organisms, will be found to be a point of great importance.

Whenever Bacteria or their allies appear in the midst of the tissues or fluids of animals or plants, two possibilities have to be thoroughly considered and excluded, before their presence can be ascribed to Heterogenesis. The two possibilities that have to be considered are these:—

(*a*) Are the bodies of plants and animals interpenetrated in all parts by visible or invisible germs of microorganisms, or are they germless?

(*b*) Have the microorganisms which may be found in the tissues or fluids of plants and animals under various conditions been

[1] All such, and many other, processes are described, and illustrated by photomicrographs, in the writer's "Studies in Heterogenesis," 1904; and many of those above mentioned are also described in the present work.

produced therein by Heterogenesis (possibly in the fluids by Archebiosis), or is their presence invariably to be ascribed to 'infection' from without?

(*a*) During the last thirty years it has been commonly held in accordance with the teachings of Pasteur, Lister and others that the tissues and fluids of healthy animals and plants are germless, and altogether free from microorganisms.

In regard to animals, however, it is clear that this position is one which cannot be accepted without very important limitations. It is obvious that microorganisms may, like other particles, get taken up from the mucous membranes of the alimentary canal and the respiratory system, and pass by means of lymphatics into the nearest glands. If they should get through these and ultimately pass into the blood, the generally accepted view is that they are speedily "destroyed" in this fluid. This view is based upon the fact that bacteriologists are never able to get evidence of the existence of microorganisms, or their germs, by the inoculation of different suitable and sterilised media, with blood drawn from a healthy man or from one of the lower animals similarly free from disease. Nothing more need be said now, as we shall have to return to this subject later on.

In regard to plants, that is, fruits and vegetables of different kinds, the case is not so complicated, and Pasteur was probably quite right in declaring that, when healthy, their cells and tissues are germless. Thus, in considering the interpretation of cases in which microorganisms are found in the interior of certain vegetables or fruits after they have been submitted to various unnatural conditions, the question will not be whether we have had to do with the wakening up of latent pre-existing germs, but rather, whether the organisms found are results of an infection that has recently been brought about—that is, during the exposure of the vegetables or fruits to the experimental conditions. And this brings us to the consideration of the second of the two possibilities above referred to.

(*b*) The second possible mode of accounting for the presence of microorganisms in the tissues of healthy animals and plants, other than by Heterogenesis, is that they have resulted from some process of infection brought about antecedently to, or during the continuance of, the experimental conditions to which they have been subjected.

It is of great importance for the proper consideration of this possibility that we should have some definite knowledge as to the powers possessed by such microorganisms as Bacteria and their allies of penetrating the tissues of plants and animals—that is, as to the means by which they are enabled to do so, as well as concerning the time needed for such an operation. Fortunately one investigation, very much to the point, can be referred to, which was made by M. C. Potter in connection with a Bacterial disease of the turnip.[1] This investigator showed that the Bacteria which cause the disease secrete an enzyme (cytase) that tends to soften and partially dissolve the cell-wall, and also a toxin which kills the cell.

The important facts made known by this research are these : the vegetable cell is only capable of being penetrated when its walls are not thick and hard originally, and after they have been extremely softened by long contact with the cytase excreted by a number of Bacteria—the need of a conjoint attack being distinctly indicated by the author who says : "Very soon the number of individual Bacteria becomes largely increased, each one contributes its share of toxin and cytase, and in a very short time these products have sufficiently accumulated to kill the first cell. . . . It is not, however, until the protoplasm has been killed and the cell-wall very much softened that the Bacteria have the power of perforating the walls and passing into the cell cavity. It would hardly be supposed that a single Bacterium, through its own excretions, could soften the wall and pierce it at one definite point after the manner of a fungus germ-tube. The extreme minuteness of the Bacteria and the rapidity of their multiplication lead them to act, as it were, in concert, and the wall becomes softened by the cumulative action of many Bacteria before the penetration of a single individual."

The mode in which a Mould infects and penetrates a vegetable cell is very similar, allowance being made for its greater size, which permits a single individual to do what can only be brought about by numbers of organisms in the case of Bacteria. This subject has been recently investigated by Nordhausen[2] while studying the parasitism of *Botrytis cinerea*. I quote from Potter, who says :—

"He has shown that the spore of this fungus excretes a powerful toxin in the initial stages of germination before any trace of the germ-tube can be detected. Its manner of effecting an entrance

[1] "Proceed. of Royal Society," vol. lxvii, p. 442.

[2] "Jahrbücher für Wissensch. Botanik," vol. xxxiii., 1899.

into a host plant is first to kill the cell by the emission of the toxin; the germ-tube then penetrates the dead cell and is nourished saprophytically upon it; with the vigour thus gained it destroys the neighbouring cells and passes from one to another without further difficulty. The fungus-hypha has the power of penetrating the cuticle, *but only in young and tender structures;* old and hardened membranes could only be entered when the cuticle had been injured, or when it [the hypha] had gained strength by special saprophytic nutrition."

These results will prove important as standards for comparison with other observations of my own which I am about to cite. We see that in the case of actual infection by Bacteria there is need for the co-operation of many full-grown active organisms (and not mere motionless germs) in order to bring about, by their secretions, the softening of the wall of every single cell that is penetrated; that some time is required for the operation; and that the softening produced must be considerable before any such penetration is possible by the moving organisms. It will be seen how very different is the state of things, in cases which I shall cite as instances of the origin of Bacteria and their allies, in the tissues of plants and animals, by a process of Heterogenesis.

It will be found, in fact, that the presence of two characteristics, wherever they co-exist, may be regarded as strongly in favour of the interpretation of Heterogenesis as against Infection, as the following remarks will show.

(1) The means adopted by Bacteria for bringing about the penetration of cells being such as are associated with *the vital processes of adult organisms*, there is no reason to think that invisible or scarcely visible germs of such minute organisms would have the power of secreting a cytase sufficient in amount to bring about the degree of softening of a cell-wall which has been found to be a necessary preliminary to their penetration. *Yet in multitudes of cases it is minute germs of Bacteria and their allies which may be seen developing within cells or tissues.*

(2) Again, the process of infection, as described by Potter, is one brought about by *active organisms* which affix themselves to a cell-wall until it becomes softened, and then succeed, by reason of this same activity, in boring their way into the cavity of the cell. On the other hand in very many of the cases in which, as I maintain, Bacteria and their allies may be presumed to be originating by Heterogenesis, what can often be seen is this—particles

becoming visible in the midst of homogeneous protoplasm; *such particles being invariably motionless* but followed soon by the development therefrom of definite Bacteria or their allies, recognisable as such by their shapes and modes of collocation.

These two points are, therefore, of great importance, and for the purpose of interpretation it should always be borne in mind that in cases of Infection by Bacteria and their allies we have to do with *adult organisms in a state of activity;* while in cases where Heterogenesis may be presumed to be occurring we have invariably, in the first place, to do with *germs and motionless organisms.*[1]

We must now see, in the first place, what cogent evidence can be obtained in regard to the origin of microorganisms within the tissues of animal organisms.

It would be useless to multiply instances. I will, therefore, first cite a single case in which the origin of Bacteria may be actually watched within the body of a low animal organism, and then turn to their mode of appearance within some of the tissue elements of vertebrates.

Evidence of a particularly convincing nature is to be obtained from the examination of a little creature low in the scale of animal life, namely, *Cyclops quadricornis*, one of the Entomostraca so commonly to be found in ponds. It may be seen from Pl. xxiv. of Baird's "Natural History of the British Entomostraca"[2] that the four pairs of abdominal feet and also the tail are furnished with a number of "plumose spines or setæ."

Examination of one of these organisms will show that within the chitinous envelope of these slender spines, which taper away to sharp points, there is nothing but structureless protoplasm to be seen (Fig. 1, A, × 700). If we take one of these little creatures, put it in a drop of distilled water, on a glass slip with a fragment of a No. 2 cover-glass on each side of it, and place over all a cover-glass, it will be found that the animal is soon killed by the weight of the latter though the fragments of glass prevent rupture of the body. We may then place the microscope slip in a Petri dish containing a thin stratum of water (so as to prevent evaporation

[1] Of course by "germs" I mean here merely minute and undifferentiated stages of the organisms in question, produced by Heterogenesis, and not the ordinary acceptation of the word, viz., a reproductive unit formed in an organism of like kind.

[2] 'Ray Society' Publication, 1850.

from beneath the cover-glass) and fixing upon one of the tail setæ (these being larger than those on the abdominal feet), we may examine it from time to time. What may be observed is this.

After an interval of two or three days (the duration depending upon the temperature of the air at the time) we may see, under a high power of our microscope, scarcely visible motionless specks gradually appear in increasing numbers in the midst of the structureless protoplasm ; and, still later, we may see some of these specks growing into Bacteria, as in Fig. 1, B, which is a representation of A after four days. At last the whole interior of the spine becomes filled with distinct Bacteria, as may be seen in C, which is from a photograph of the same spine on the sixth day—the temperature during these days varying from 70–75° F. Later still, all the Bacteria, previously motionless, begin to show active swarming movements.

In such a case it is clear we have to do with no process of infection from without, but with a *de novo* origin of Bacteria from the protoplasmic contents of the spines or setæ. The fact that they appear in these situations as mere separate, motionless specks, and gradually take on the forms of Bacteria (also motionless at first), is, as I have previously indicated, just what we might expect if they had actually taken origin in the places where they appear. On the other hand, such a mode of appearance is totally opposed to what might be expected, if the microorganisms had obtained an entry from without, through the tough chitinous envelope of the spines. It will be observed also that the apparent origin and mode of appearance of Bacteria here is precisely similar to what is to be seen, when a film of a suitable organic fluid is watched under the microscope, and Bacteria gradually make their appearance therein (see p. 52).

I pass now to what may be regarded as another absolute proof of the heterogenetic origin of Bacteria, as convincing as that which will be subsequently shown to occur within the closed cells of certain vegetables.

I have already pointed out that in many parts of the bodies of man, and of higher animals generally, microorganisms are known to exist in abundance. This is the case, for instance, throughout the whole length of the alimentary tract, and throughout a considerable extent of the mucous membranes of the respiratory tract. It is clear, also, that some of the microorganisms may be taken up from these mucous membranes by lymphatics, and if they pass the

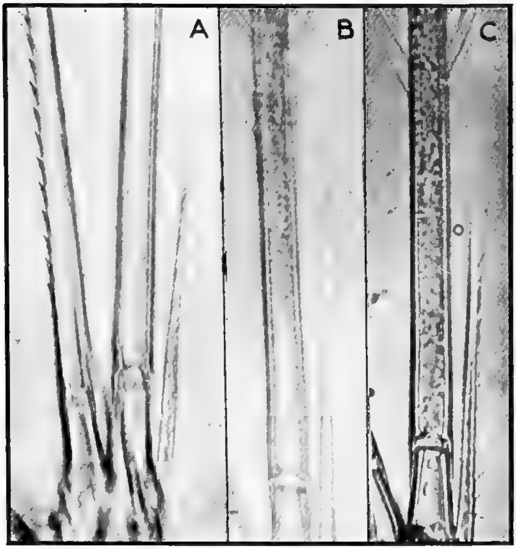

Fig. 1.

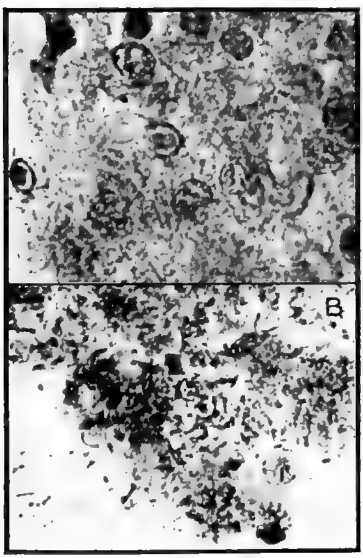

Fig. 2.

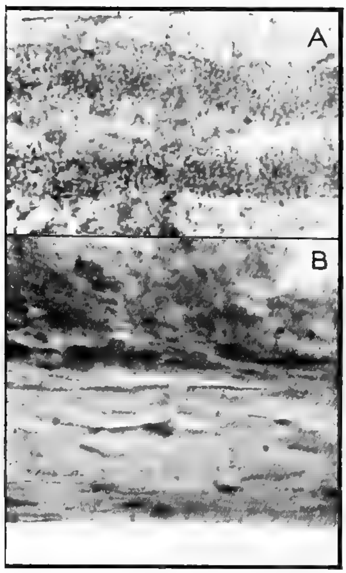

Fig. 3.

To face p. 166.

nearest lymphatic glands, some of them would ultimately find their way into the blood. When there, the view generally accepted is, as I have said, that the Bacteria and their allies are at once "destroyed." The blood of healthy animals is declared to be germless, and much importance is attached to the germicidal qualities of this fluid.

It has, however, been held by Tiegel, Burdon Sanderson and others that though the blood is germless, "those parts of the animal body which are in closest proximity to absorbing mucous membranes are most liable to be found pregnant with microphytic life when tested by suitable methods."[1] Their experiments showed indeed that such organs as kidney, spleen and liver, when removed from the body of a healthy animal immediately after its death, and suitably treated, could always be made to reveal the presence of microorganisms.

But cutting out portions of internal organs of recently killed animals, enveloping them with superheated paraffin, and then placing them in an incubator at a suitable temperature, followed by the finding of swarms of Bacteria in the central red and uncooked portions, is not a method that can possibly give us certain information as to the mode of origin of the organisms found. It may be, as Burdon Sanderson and others concluded, that the organisms found came from "germs which existed and retained their latent vitality in the living tissues"; but it is, at least, equally possible, as I maintain, that they may have had a heterogenetic origin within these tissues themselves.

It seems perfectly clear that experiments of this kind, if carried no further, could teach us nothing decisively; that their results, in fact, are of no more value than those that may be obtained by the examination of the brain and its membranes three or four days after a healthy animal has been killed. There also swarms of microorganisms would be found, as I can testify; and if bacteriologists are right that organisms and their germs are, as they say, "destroyed" in the blood, we could only conclude that the organisms so found must have been produced by archebiosis or by heterogenesis. It could not reasonably be said that the organisms naturally present in the intestinal canal have been enabled to spread through the body so as to reach the most remote parts *after death*—since many of the organisms are

[1] "British Medical Journal," 1875, vol. i., p. 199.

motionless, and others exhibit mere to-and-fro movements of a non-progressive character. The blood, again, has ceased to circulate, so that this fluid, germless during life, cannot, after death, be considered to act as a carrier. But if the organisms themselves cannot make their way through the tissues and travel far within brief periods, and if no carrier exists, they must have been born in or near many of the sites in which, after death, they are speedily to be found.

Speaking of the experiments made by himself Burdon Sanderson wrote as follows[1]: "Under the conditions I have described to you, it seems to me quite impossible to suppose either that germs could penetrate to the organ from the outside, or that any germ encountered by the organ in its transference from the body of the animal to the basin could escape destruction. If, therefore, Bacteria be found, they or their germs must have been there before the organ was plunged into the hot liquid. . . . The results of all the experiments whether with liver or kidneys was the same. The soft red kernel of uncooked tissue at the middle of the organ always contained Bacteria, the vigorous development of which was indicated by their large size, countless numbers, and active movements. To my mind the experiment is conclusive."

In reference to this, it is right to say that similar results have been obtained by other investigators using either similar methods, or methods equally trustworthy. Such experiments have been made by Tiegel, Billroth, Nencki and Giacosa, Horsley and Mott, as well as by myself.

As I have previously intimated, the finding of organisms under these conditions is a fact so important, in view of the theories of Pasteur and Lister, and the general belief as to the germicidal qualities of the blood, that the results require to be most carefully scrutinised.

To postulate the presence of "latent germs" in these abdominal organs and to assign as a reason the close "proximity to absorbing mucous membranes," and nothing else, surely cannot be regarded as a full and satisfactory explanation. It assumes, without proof of any kind, that microorganisms, even in healthy animals, are constantly making their way out through the walls of the intestine, and wandering promiscuously into this or that organ, only in the end to lapse into a condition of "latent vitality." Could anything in the way of explanation be more gratuitous and unsatisfactory?

[1] "British Medical Journal," 1878, vol. i., p. 119.

With the view of throwing light upon this important question, by the production of actual evidence, I obtained a sheep's kidney from a recently killed animal, and saw a coating of fat nearly an inch thick stripped from it. The whole organ was then left to soak for four hours in a two per cent. solution of chromic acid, so as to destroy any organisms by which its surface may have been contaminated. It was then removed and placed in a bottle still wet with another germicidal solution, namely, a ten per cent. solution of formalin. The screw-top having been fixed so as to prevent evaporation, the bottle was transferred to an incubator and left at a temperature of 76° F. for thirty-six hours.[1] When the organ was cut the chromic acid solution was found to have discoloured it to a depth of about a quarter of an inch, but within that margin the kidney substance was red and only slightly softer than natural. There was no distinct odour of putrefaction. A small portion of the organ was cut out and teazed in a drop of a weak solution of gentian violet, and fragments, after a short interval, were carefully examined under the microscope. A comparatively small number of Bacteria were found free, between the separated and broken-up cells, and the cells were densely filled with granular matter, as may be seen in Fig. 2, A (× 700), but it was impossible to identify with certainty any of the granules as germs of Bacteria. Sections that were made and carefully stained gave no more definite results.

Another sheep's kidney from a freshly killed animal was therefore obtained and treated in the same manner except that it was left in the incubator at 76° F. for three and a half days, instead of merely thirty-six hours. When the organ was cut through it was deeply stained at the circumference, as before, with the chromic acid; but the red tissue within was much softer, and the odour was most offensive and putrid. Portions of the organ were at once put into a ten per cent. formalin solution with a view to obtaining sections therefrom; but a minute portion was cut off, as before, and teazed in a drop of gentian violet. On examination with the microscope, after a brief interval, I found the fragments of the tubules and kidney cells full of Micrococci which had taken the stain well, together with numbers of figure-of-eight organisms and short chains (Streptococci) such as are shown in Fig. 2, B (× 700).

[1] I was using the incubator for other purposes at this temperature, and therefore did not make use of a higher temperature.

In a few days Dr. J. S. Collier kindly sent me a number of sections, stained and unstained, which he had been good enough to cut from the portion of the kidney in the formalin solution, which I had handed over to him. Some of the sections sent had been stained with methylene blue or with logwood, and then mounted in balsam; while I stained some of the plain sections with gentian violet, and subsequently mounted them in glycerine. On the whole, rather more details could be made out with these latter sections than with the specimens mounted in balsam.

Every section, however, showed, inside the area which had been affected by the chromic acid, that almost each cell within the renal tubules was full of developing or actually developed Bacteria. The former appeared as mere cocci-like particles which had taken the stain like the developed Bacteria, both being situated in the midst of cell granules comparatively unstained. The organisms were distributed through the substance of the cells, but often seemed to be most abundant in the half of the cell next the wall of the tubule. In a few of the tubules in the pyramidal portions of the kidney the organisms had developed more abundantly, so that the cells were filled with a dense mass of Micrococci such as may be seen in Fig. 3, A (× 700). In sections through blood-vessels also a moderate number of Bacteria were seen mixed with the blood-corpuscles.

Here, then, it is clear we have again the kind of appearances which we have a right to expect if the microorganisms had developed in the epithelial cells of the kidney by heterogenesis. We have the cells full of particles developing into fully formed Bacteria, and, what is more important still, we have them within almost every epithelial cell to be seen in the sections.

From the point of view of the wholly inadequate suggestion as to organisms being found in such an abdominal organ as the kidney, by reason of its "proximity to absorbing mucous membranes," it may be well to recall the fact that the sheep's kidney is encased in a mass of fat from half an inch to an inch in thickness, and that, as an additional barrier separating wandering microorganisms from the epithelial cells of the organ, there is still the thick and tough capsule which I have thought it not useless to show in Fig. 3, B (× 700)—where it is represented, under the same magnification, by the lower half of the figure. Let any one compare M. C. Potter's description (p. 163) of the mode in which Bacteria are enabled to

effect an entry into a single vegetable cell, and then let him imagine what an army of Bacteria would be needful, with all the cytase they could excrete, to get through such a tough and thick layer as that presented by the fibrous capsule of the kidney. But surely the whole notion as to such a mode of infection of the kidney and other abdominal organs is too absurd for serious consideration.

We are, then, driven back to inquire whether it is true that the blood is germless, and whether it has, in reality, the bactericidal qualities with which it is credited. I have no evidence whatever to oppose to these beliefs; nor is it easy to see, even if bacteriologists generally should be wrong in their views as to these points, how it would suffice to explain the development of Bacteria within all the cells of a kidney treated in the way I have mentioned.

It is perfectly certain that the blood of healthy persons does not contain any appreciable number of active Bacteria. But are bacteriologists right in supposing that such Bacteria as get into the blood stream are "destroyed"? May they not rather be reduced to a condition of latent vitality? Their answer to this is, that if it were true, the organisms would be capable of revealing their presence when suitable media were inoculated with them and subsequently exposed to proper incubating temperatures. And it is the negative results of all such experiments with the blood of healthy animals that confirm them in their belief as to the germicidal qualities of the blood.

Nor, in fact, if the blood were assumed to be full of latent germs of Bacteria would it be easy, as I have intimated, to see how that would enable us to explain the development of Bacteria within almost every epithelial cell in the kidney referred to. Could organisms reduced to a condition of "latent vitality" penetrate the walls of the capillaries, and thence migrate into all the cells of a kidney tubule? The notion is again too preposterous to be entertained; so that we are compelled, by evidence of a most convincing character, to admit that the Bacteria have in reality been born in the individual cells of the kidney—we are compelled to believe that heterogenesis has, in fact, been taking place here, as in the other instance previously cited.

We may turn now to see what evidence is forthcoming concerning the heterogenetic origin of Bacteria and their allies within the tissues of plants and other vegetal organisms.

Before citing some experimental results bearing upon this point I may mention that Bacteria may often be seen developing within the living cells and filaments of various *Algæ*. In Vaucheria and in Spirogyra this is commonly to be seen where the plants have been kept in unnatural conditions for a time ; shut up, for instance, either in a cupboard or within a stone pot. In the case of Vaucheria they may often best be recognised in and near the growing points of the filaments where the microbes show themselves first as mere motionless specks, which gradually develop into Bacilli, and after a time take on an active existence. In the filaments presenting these appearances the wall may appear quite healthy, showing no signs of softening, nor is there any indication whatever of the penetration of Bacteria from without.

The same kind of thing is often to be observed within the thick-walled resting spores both of Vaucheria and of Spirogyra. There is the appearance of motionless particles in some part of the spore, the appearance of Bacteria in the midst of these particles, and the gradual assumption by the Bacteria of swarming movements.

Again, I have frequently seen a development of motionless Micrococci and Bacilli taking place inside the thick wall of a Nitella cell, between it and the chlorophyll layer. Yet the normal cyclosis would be still going on within this cell, showing that there could be no apertures or solutions of continuity of any kind ; and all the microorganisms to be seen in different stages of development in this layer were quite motionless.

I have also endeavoured to throw light upon this question by repeating, with variations, some of the experiments of Lechartier and Bellamy, by which they studied the fermentation that occurs in various vegetables and fruits when shut up within hermetically sealed vessels. They showed that the oxygen of the air was soon consumed by the vegetables or fruits ; the cells of which then began to break up sugar, to give off carbonic acid, and to produce alcohol and acetic acid. They came to the conclusion that this fermentation might certainly occur without the production of the alcoholic ferment.[1] They, in fact, adopted Pasteur's view that the formation of alcohol in these cases was due to the altered activity of the cells of the fruit, which, in the absence of free oxygen, act after the fashion of ferments. In a later communication,[2] however, these investigators stated that in their experiments with potatoes

[1] "Compt. Rend.," 1872, ii., p. 1203.
[2] *Loc. cit.*, 1874, ii., p. 1006.

and beet-root, while alcohol and carbonic acid were produced in the same way as with the fruits, and the alcoholic ferment was absent as before, Bacteria of different sizes were invariably found in the acid fluid which impregnated the softened tissues of the vegetables in question. No details on this point were given, and the authors do not appear to have made any further observations on the subject; nor did Pasteur offer any reply to such statements, though he had previously been working at the same subject himself.[1]

Having determined to endeavour to obtain some more definite information as to the appearance of Bacteria in this way, I have, during the last two or three years, made various experiments, in which small *Potatoes*, after being carefully washed, were allowed to soak for a time in different germicidal fluids. First of all, I employed a solution of mercury bichloride (1 : 2000); while later, after the preliminary washing, the potato was allowed to soak in a five per cent. formalin solution. The screw-top bottle in which the potato was placed was also thoroughly washed out with one or other of these fluids. In these cases organisms were found within, but also, after a time, on the surface of the potatoes thus treated; so that these particular experiments and methods were rejected as not yielding trustworthy results. This was necessary because, at a rather earlier date, Pasteur had stated that, in experiments which he had made with fruits no ferment organisms ever appeared. He declared again that the tissues of healthy fruits and vegetables were germless, but intimated that, unless care was taken, they might make their way in from without.[2]

Subsequently, I used a stronger solution of formalin, and have never since found organisms on or near the surface, though they have often been found within cells in the central portions of the potato. I will now, therefore, give brief details of some of the most successful of these experiments.

In July, 1901, a small new potato, after being well washed, was allowed to soak in a ten per cent. solution of formalin for ten minutes, in a small screw-top bottle, and during this time the fluid was frequently shaken so as to cover the whole inner surface of the bottle. At the expiration of the time named, the top was unscrewed, the fluid poured out, and the top then tightly refixed, leaving the potato itself and the inner surface of the bottle wet

[1] "Compt. Rend.," 1872, ii., p. 788.
[2] *Loc. cit.*, 1872, ii., pp. 788 and 981–2.

with the formalin solution. The bottle was subsequently left in a cupboard for seventeen weeks, the temperature of which, for a long time, remained about 70° F., though it afterwards fell to 50° F.

When removed from the bottle at the expiration of this time the potato was found to be quite firm and not at all shrunk. On section it was seen to be discoloured to a pale earthy tint, with mottlings here and there of a rather darker colour. The cut surface was moist and had a distinctly acid reaction, and there was not the least sign of softening or disintegration anywhere. Thin sections having been made, they were shaken up in a small tube with distilled water, so as to get rid of the starch-grains from many of the cells, and the sections were subsequently allowed to soak for two hours in some of Westphal's mastzellen stain, diluted with two per cent. formalin.

On microscopical examination of these sections, groups of Bacteria were found in large numbers of the cells, though not in those near the surface. The contents of one of these cells is shown in Fig. 4, B (× 500); some of the Bacteria were free and others were in, or lying on, the primordial utricle; but, as I have usually found with microorganisms in such situations, they were not appreciably stained. Some cells, which did not contain obvious Bacteria, showed plenty of minute cocci-like bodies on the surface of the primordial utricle, also not taking the stain, which probably represent early stages of the Bacteria (Fig. 4, A, × 700).

Another larger potato, about two inches in diameter, was treated in exactly the same way as the last in September, 1901, and after the bottle was finally closed, it was left on the surface of an incubator at a temperature of about 80° F. for seven weeks.

When examined the potato was not found to have shrunk, or to be appreciably altered on the surface. On section, it was moist, of acid reaction, and showed as before a pale earthy colour with rather darker mottlings in places.

Sections were made and treated in the manner previously indicated, and, on examination, multitudes of Bacilli were seen here and there in cells in all parts of the section except for about one-fifth of an inch from the surface. In places, also, there were fine mycelial filaments containing spore-like bodies. Some of these Bacilli took the stain fairly well as may be seen in Fig. 5, A (× 500), in which the two kinds of organisms are shown. In or on the primordial utricle also there were multitudes of very delicate interlacing filaments (? Bacilli), containing an abundance of spores

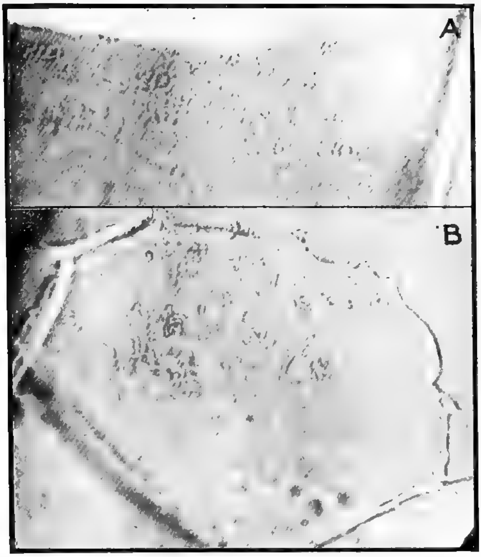

FIG. 4.

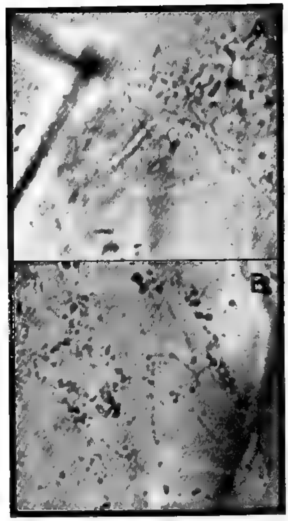

FIG. 5.

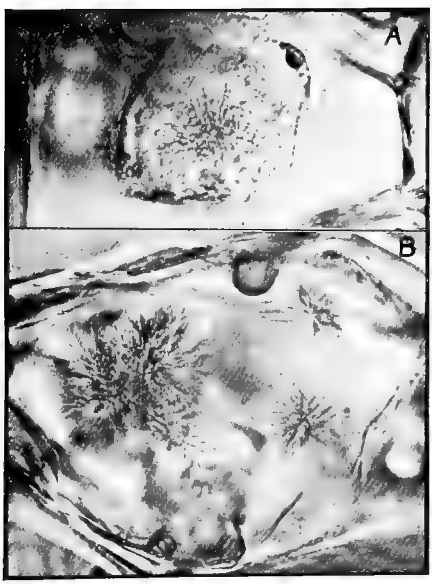

FIG. 6.

To face p. 174.

which had taken the stain freely, as may be seen by Fig. 5, B (× 500).

At the end of March, 1903, I made another slight variation in the conditions with a small new potato. After careful washing, it was placed in a small *tin* with a very tightly fitting cover and allowed to soak in ten per cent. formalin for twenty minutes, the fluid having been shaken about several times so as thoroughly to wet the whole internal surface of the tin. The fluid was then poured off, leaving the surfaces wet with the solution as before; the tin cover was very tightly jammed down and the vessel was placed *within* a copper incubator at a temperature of 75° F., and allowed to remain there for eight weeks completely cut off from all light.

When taken out and examined, the cut surface of this potato presented just the same characters as the others; the whole substance was firm throughout, there was no shrinking, and the central portion was rather darker than the other parts which showed the usual mottling.

Sections were made, shaken up in distilled water in a small tube as before, and then placed for a short time in a dilute gentian-violet solution. On microscopical examination a large number of the cells scattered throughout the sections were found to show the most delicate branching tufts of what seemed a new kind of microphyte, looking like a species of Cladothrix, which had taken the stain slightly, such as are shown in Fig. 6, B (× 500). These tufts were mostly seen to be sprouting from the internal, or else from the external, surface of the primordial utricle, where it had shrunk away from the cell-wall. At first no Bacteria were found, only tufts of this kind, but a subsequent thorough examination of other sections revealed Bacteria in abundance, and in some cells dense aggregations of them such as are shown in Fig. 6, A (× 500). It seems probable that the tufts are really aggregates of Bacilli growing out from such masses of germ-like bodies. In other cells these germ-like bodies were seen originating from the primordial utricle, as shown under a higher power in Fig. 7, A (× 700), while in B such bodies were to be seen giving rise to filaments which were ramifying over the surface of the membrane.

A few experiments have also been made with small *Turnips* about two inches in diameter, to two of which I will now refer.

A perfectly sound turnip of the size mentioned was, in November, 1901, first well washed in water and then allowed to soak in a

screw-top bottle in a ten per cent. formalin solution for ten minutes. It was subsequently treated in exactly the same manner as the potatoes had been. After the top of the bottle had been tightly screwed on, it was left on the top of the incubator at a temperature of about 80° F. for seven weeks.

On examination, at the expiration of this time, the upper two-thirds of the turnip was found to be somewhat shrivelled. The odour of the bottle was disagreeable and pungent, though slightly aromatic and spirituous. The odour was so strong that it did not seem likely the shrivelling was due to evaporation, which had been caused by the screw-top not being quite air-tight.

On section, the rather shrivelled upper two-thirds was found to be much discoloured and honeycombed; the lower third being much less so. Sections were made and soaked in dilute mastzellen stain; and on examination cells here and there, not continuously, but in the upper and lower portions alike, were found to be crowded with very minute Bacteria, most of which took the stain only slightly. In Fig. 8, A (× 500), a large aggregate of these organisms is to be seen, with others scattered about over contiguous portions of the section.

Another small turnip of the same size as the last was, on the same date, after being well washed, put into a screw-top bottle and placed on a small earthenware pot, in order to protect it from six drachms of pure formalin which had previously been poured into the bottle. The top was then tightly screwed on, and the bottle was placed on the incubator, by the side of the other (at a temperature of about 80° F.), where it remained for eight weeks: the turnip being in an atmosphere saturated with formalin vapour.

On examination this turnip was likewise found to be slightly shrivelled, and it was rather soft and doughy to the touch. On section the colour was almost natural except for a depth of about one-third of an inch round the periphery, where it was slightly discoloured, and in the centre where there was a small area about one quarter of an inch in diameter which had a rather gelatinous appearance.

Two sections through this central region and its neighbourhood were made, and then soaked in dilute mastzellen stain. On microscopical examination they were found to contain moderately large Bacteria, mostly in small groups, in a large number of the cells; though here and there large masses of Bacteria were found, such as are shown in Fig. 8, B (× 500). In many of the cells the

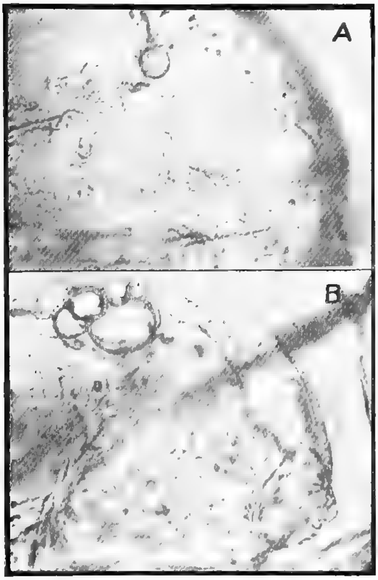

FIG. 7.

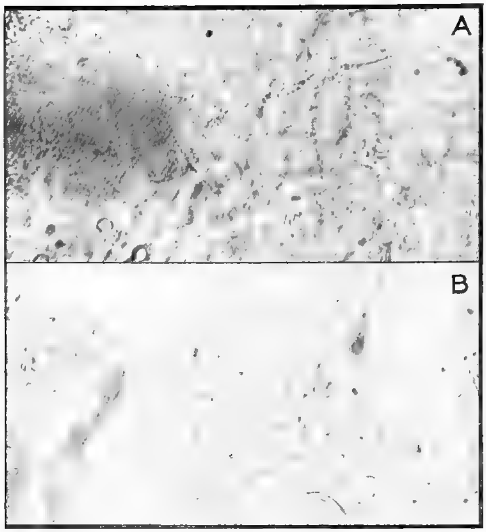

FIG. 8.

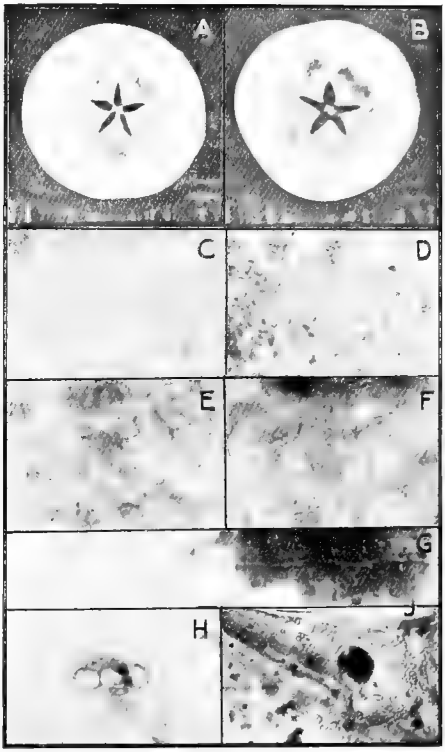

FIG. 9.

To face p. 176.

Bacteria seemed to be developing in, and on the surface of, the primordial utricle, and also within, and on the surface of, the nuclei of the cells.

I have made only one experiment of this kind with an *Apple*; and, in this case, a rather small but thoroughly sound specimen was placed in a screw-top bottle, standing on a small earthenware pot as before, so as to remove it from contact with some pure formalin which was placed at the bottom of the bottle. The top was tightly screwed on, and the bottle was then placed in a cupboard, where it remained for eight weeks, the temperature of the cupboard varying during this time between 70° and 56° F. The apple was thus left, as the turnip had been, in an atmosphere saturated with formalin vapour.[1]

On examination, the surface of the apple was found to be hardened, and, on section, irregular patches of brownish discolouration were seen. Otherwise nothing abnormal was observed.

Microscopical examination of an unstained section showed, in a few of the cells, a small Fungus mycelium. Specimens of the same kind were found in cells in different parts of the section, though in the majority of them nothing of the kind was met with. When present the growth seemed to start from the cell-wall close to the nucleus, if not from the nucleus itself. No Bacilli were seen; but in some cells there were found what, judging from their uniform size and mode of arrangement, appeared to be a number of Micrococci on the primordial utricle. As usual, they scarcely stained at all with carbo-fuchsine.

I have now to record an interesting case of spontaneous change in some apples, which occurred under the following circumstances. Early in the winter of 1902 I received from a friend in America a case of very choice Canadian apples. The case had a separate cardboard partition for every apple, and they were all in excellent condition. Some of them were kept to a date as late as the second week in January of the following year. From about the middle of December I noticed that many of these apples, when cut through the centre, showed a brown discolouration beginning at a number of separate points around the periphery of the core, as may be seen in Fig. 9, A and B ($\frac{1}{2}$ nat. size). An early stage of the change is

[1] I have not had many successes with this method, and do not recommend it, especially as the soaking for a time in ten per cent. formalin has proved to be perfectly sufficient to guard against external contamination. It is difficult to tell how far the formalin vapour penetrates into the substance of fruits or vegetables left in an atmosphere saturated with it for many weeks.

shown in A, and a more advanced stage in B. In all other respects the apples were perfectly sound and of a delicious flavour, and none of those eaten anterior to the date mentioned showed any unusual appearance. Towards the end of December the above photographs were taken; and on consulting Lindley's "Vegetable Kingdom," it became plain, from a figure there shown (p. 559), that these points of change occurred at the junction of the ovarian and calycine portions of the fruit.

I examined portions of the altered tissue under the microscope, fully expecting to find some Mould as the cause of the change. But, much to my surprise, after a tolerably careful examination, I was unable to feel sure that organisms of any kind were to be found in the tissue which had become thus altered. Subsequently I tried to stain some sections, and made a still more careful examination, with the result that I found on, or in, the primordial utricle of many cells, cocci-like bodies looking like the germs of microorganisms. But as their nature seemed doubtful I took two of the apples to Dr. Allan Macfadyen and asked him kindly to see whether any microorganisms could be developed from this altered tissue of the apple. On January 5th he wrote to me, as follows: "I was unable to detect the presence of Bacteria in the Canadian apples you left here, by microscopical examination. I accordingly made a number of subcultures, but in no instance have I succeeded in obtaining a growth." [1]

I had by this time only three of the apples left, so I placed them in the incubator at a temperature of 76° F. and there left them for eight days. On section two of them were found to present the brown discolouration in the usual situations to a well-marked extent. Some portions of the brown tissue were broken up with needles, placed in a dilute solution of the mastzellen stain, and were afterwards submitted to careful examination with the microscope. There was certainly a very distinct increase of the cocci-like bodies in the primordial utricle, remaining unstained, as in Fig. 9, C (× 700), though all the other granules in the cells had become strongly stained. In some places the cocci were seen in distinct rows, branching and crossing one another as in J (× 375),

[1] Some time previously Dr. Nabarro, of University College, had been similarly unsuccessful in obtaining growths from a potato which had been treated in the manner I have detailed on p. 173, and in which organisms seemed to be present at an early stage. Such lack of success after trials with a few culture media is, of course, far from disproving the presence of microorganisms.

in the neighbourhood of the letter, so that they looked like spores within minute filaments.[1]

A further careful and prolonged examination revealed the fact that very many of the cells showed, in whole or in part, on or in the lining membrane, the cocci-like bodies as in C, though in other cells there were none of them. There was often a tendency for these bodies to arrange themselves in rows (as in E and J); and in places to grow into delicate filaments (as in D and F). Such filaments were also seen occasionally crossing the cavity of the cell, and having spore-like bodies at intervals. A few larger filaments or hyphæ, such as G, were likewise seen together with Toruloid corpuscles, as in H. The spore-like body in G, from which the hypha had developed, was only a little larger than one of the cocci-like bodies to be seen near the lower left corner of C. Any doubts as to the reality of these latter bodies being embryo Bacteria may be set at rest by comparing them with what is shown in Figs. 4 and 7. All the organisms found here, as with those shown in the figures above-mentioned from other vegetable cells, were similarly obstinate in refusing to stain with all ordinary dyes.

Although the first examination of these apples showed, therefore, only very doubtful organisms, or none at all, a prolonged search has made their presence abundantly clear, and has shown that the spontaneous changes occurring in so many distinct foci in the very midst of the tissue of the apples has been correlated with the origin and development of microorganisms.

I have also made a very few observations on *Tangerine Oranges*, to two of which I will refer. In February, 1901, two of these oranges were placed in a screw-top bottle and soaked in a ten per cent. solution of formalin for fifteen minutes, the fluid being also shaken about several times so as to wet the whole inner surface of the bottle. After the fluid was poured off and the cover tightly screwed on, the bottle was placed in a cupboard for fifteen days, the average temperature of which was about 50° F.

When the first of these oranges was cut through in a longitudinal direction, a slight mouldy odour was at once perceived, and in the central white tissue, and around the pips, there was a greenish black mass of mould. This was strictly confined to the central parts of the orange, and nowhere came within three-fourths of an inch of the surface. The skin generally was perfectly sound, though it

[1] Unfortunately this particular photograph was taken at a low magnification but C, and each of the others except A and B, was taken at 700 diameters.

had become hard and was of a slightly brownish colour from the action of the formalin. Microscopical examination of the more peripheral parts of the orange also showed no mycelial filaments or organisms of any kind.

The other orange in the same bottle showed no organisms, either to the naked eye or on microscopical examination.

Soon afterwards, two other Tangerine oranges were treated in the same way and subjected to similar conditions, except that they were left in the bottle for a much longer period—that is, for five and a half weeks instead of only fifteen days. About five days before the bottle was opened one of the oranges was seen to show a patch of dark colour on one side, and when it was subsequently cut open longitudinally all the central white tissue was found to present an altered appearance, being of a rather dirty white colour; and on microscopical examination it was found to be densely infiltrated with a delicate Fungus mycelium. The seeds were discoloured, and the mycelium was also found to extend into the yellow substance of the orange. In one place there was a patch of a blackish colour, and this was found to have grown into the rind of the orange at the point where the discolouration on the surface was seen. It had not, however, actually reached the external surface, it had evidently grown from within outwards, and the surface of the orange, here and elsewhere, showed no trace of Mould of any kind.

The companion orange again showed no organisms either internally or externally.

There is no means of accounting for Mould springing up in the interior of an orange by infection from without. In a memoir entitled "Recherches sur la pourriture des fruits"[1] Davaine points out that in fruits, such as the apple, the pear and the medlar in which there is an open calyx, "le tube calicinal peut conduire les spores ou leurs filaments jusqu'au centre du fruit. C'est ainsi que se produit le blettisement,[2] qui n'est autre chose qu'une pourriture;" but the process of rotting, he says, *is always external* "chez les fruits qui sont partout recouverts d'un épiderme, tels que le citron, l'orange, et les fruits à noyau."

[1] "Compt. Rend.," 1866, pp. 277 and 344.

[2] That is, the mellowing process that occurs in pears and medlars more especially. Further on in his paper Davaine says he has "recently recognised that this latter (*blettisement*) may take place where spores are excluded, and in the absence of any mycelium."

In the case of the apples to which I have referred there was clearly no such process of infection from within as that to which Davaine refers. In the Canadian apples the change occurred simultaneously in many points almost as much removed from the seed cavities as from the surface of the apples ; and a comparison of what was found in the primordial utricles of the cells, with what has been found in similar situations in the potatoes that have been referred to, leaves little room for doubt that what are shown in Fig. 9, C, D, etc., are really germs of microorganisms. While in the other apple delicate Fungus mycelia were found springing up within various isolated cells, in the midst of the substance of the fruit.

Again, the presence of the Bacteria and other organisms within the cells of the two small turnips, and the different potatoes that have been referred to, are equally incapable of being accounted for by any process of infection from without. There is absolutely no relation between what I have found in these cases, and an actual process of infection such as M. C. Potter has described (see p. 163). We have to do, in fact, in the cases that I have cited, with motionless germs of microorganisms arising *de novo* in or on the substance of the primordial utricles of isolated cells, having intact walls, and scattered throughout the substance of the potatoes and the turnips in question—in all parts, that is, except in the superficial portions that have been saturated with the germicidal fluid in which the tubers had been for a time soaked. [1]

As I have previously pointed out (pp. 162, 173) the existence of "latent germs" in the substance of healthy fruits and vegetables is not assumed—it is, in fact, expressly denied. Hence the great weight to be attached to the preceding observations as evidence that the various microorganisms found within the cells of the fruits and vegetables with which experiment has been made — like those in the cells of the kidney and in the spine of the Cyclops —have actually originated there by heterogenesis.

[1] In these cases the organisms often have to be long and carefully searched for. A perfunctory examination would almost certainly lead to the statement that no organisms were present.

CHAPTER X

THE HETEROGENETIC ORIGIN OF FUNGUS-GERMS, OF MONADS AND OF AMŒBÆ FROM MINUTE MASSES OF ZOOGLŒA

SOME of the changes to which reference is now to be made were briefly described by me in 1870,[1] and again more fully in 1872.[2] Although the changes then referred to were of a very remarkable nature, and were declared to be of such a kind as to lead to the production of flagellate Monads, of Amœbæ, and of Fungus-germs from aggregates of Bacteria imbedded in a "more or less abundant, pellucid, gelatinous material," they seem to have attracted little serious attention, probably because they were so surprising as to be regarded as incredible. I am not aware that any bacteriologist in Europe, America or elsewhere has ever repeated my observations. They seem absolutely wedded to their strict laboratory methods, and seemingly prefer to have dealings with nothing but pure cultures and sterilised media.

I have recently devoted much time to a further study of these changes occurring in, what has been named, the "proligerous pellicle," and have been able not only to confirm the accuracy of the results previously recorded but also to considerably extend them. [3]

Bacterial scums are well known to be exceedingly common in ditches and ponds—that is, in nature's laboratories—and it is a matter of great interest to know what goes on therein. Some light may be thrown upon this subject by making infusions or macerations from cut fragments of various plants, and then examining, at

[1] "Nature," No. 35, June 30, p. 172.

[2] "Proceed. of Royal Soc.," 1872, vol. xx., p. 239, and "The Beginnings of Life," vol. ii., chap. xvii.

[3] At the time when my previous papers were published, very little was known concerning *Zooglœa* ; and that term was not employed in describing the constitution of the pellicle and of the "embryonal areas" occurring therein—though the latter were referred to as aggregates of Bacteria which had formed "around themselves a certain amount of pellucid, gelatinous matter."

different periods, the scum or pellicle that forms on such fluids. What I have now to say will refer almost exclusively to infusions made from hay. The hay employed may be either fresh or old, but it does not do to substitute for hay mere unripe grasses. I have elsewhere shown how remarkably different are the products derivable from living, unripe grasses and from ordinary hay.[1]

In making such an infusion I have been accustomed to cut the hay into short pieces, to place these in a little beaker, and then to add water so as well to cover the fragments. After maceration for three or four hours at a temperature of about 86° F. (30° C.), the infusion has been filtered through two layers of the finest Swedish filtering paper into another small beaker. In this way all but the smallest particles, 1/15,000 of an inch or thereabout, will be excluded. For observation of the changes now to be described it is best that the bacterial scum, which soon forms on the surface of the fluid, should be very thin, therefore the infusion should not be too strong, and the depth of the fluid ought not to be more than about one inch.

As all the processes with which we are concerned go on almost, if not quite, as well in the dark as in the light, one simple plan is to filter the infusion into small one-ounce earthenware pots, over which the covers are then placed till the time comes for the examination of their contents. If three or four pots are charged at the same time, they may be opened at will on successive days, or some may be exposed to one temperature and some to another.

Otherwise the infusion may be left in a shallow open glass vessel, exposed to light, and beneath a bell jar so as to exclude dust. About 65° F. (18° C.) is a favourable temperature, and, when thus exposed, in about twelve hours or so the fluid begins to show slight turbidity, and as this increases the light sherry-coloured fluid becomes gradually paler. In from twenty-four to thirty hours a thin almost invisible scum will have formed upon the surface of the infusion, often in the form of a coherent elastic membrane, and, somewhere between thirty and forty hours, multitudes of small zoogloeal masses, such as I formerly termed "embryonal areas," of varying size and shape will have formed, and will be found to be imbedded in this thin scum or pellicle. These go on increasing in size and growing in number till at last they may occupy more than one-half of the pellicle. Fig. 10, A (× 100) shows such masses, more

[1] "Studies in Heterogenesis," 1904, p. 87.

than usually abundant, found on a hay infusion on the fourth day. They are rendered more distinct owing to the ground-work of the pellicle having been stained by a weak eosin solution. B (× 500) shows a portion of such a mass highly magnified.

When we attempt to remove a small portion of the early and almost invisible scum on the tip of a sterilised scalpel, we may often see that its point will very perceptibly depress the surface before it breaks through the membrane. This is especially the case when infusions of a greater depth than those I now recommend are employed, and then, also, each day that passes adds to the thickness of the pellicle, till after seven or eight days have elapsed it may have been converted into a comparatively thick pulpy layer, owing to continued accretions of fresh Bacteria from below. After this period, or even before, the superficial layer of the pellicle may gradually become more and more brown, while rounded, or branched and elongated masses of zooglœa project from its under surface. Ultimately, after three or four weeks, the pellicle, if left, may break away and in part sink to the bottom of the vessel.

Such are the changes to be met with during the formation and growth of a pellicle on the surface of a hay infusion made and exposed in the manner I have described. One of the most notable points in connection with it is the fact, that from a very early period the Bacteria, which thus aggregate into a scum at the surface of the fluid, where they are freely exposed to air, excrete a transparent jelly-like or glœal substance by means of which the constituent units of this scum become blended into a thin elastic membranous layer.

As may be supposed, when we examine some of the turbid fluid, or the thin scum that first appears, Bacteria of several different kinds are pretty constantly met with; but, for the most part, with hay infusions, it is Bacilli which largely predominate, such as may be seen in Fig. 24, A (× 500). Toruloid corpuscles are decidedly rare, and no Monads, Amœbæ, or Ciliates are ever to be seen at this early stage. The mode and times of their appearance will be presently dealt with. In the first place, however, I would emphasise the fact that the researches which I am about to detail have no pretence to be conducted in ways that are proper and usual in the great bulk of bacteriological inquiries. We start here, confessedly, with a mixed association of Bacteria, tending to aggregate in a promiscuous manner on the surface of the fluid,

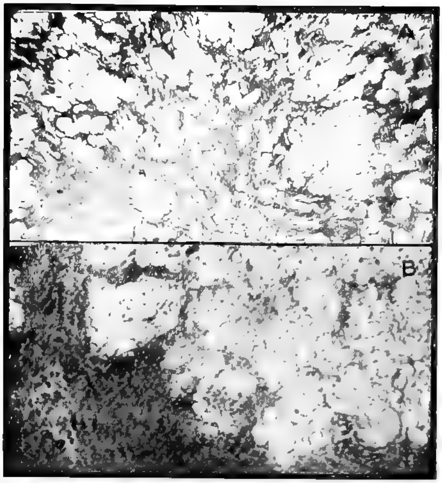

FIG. 10.

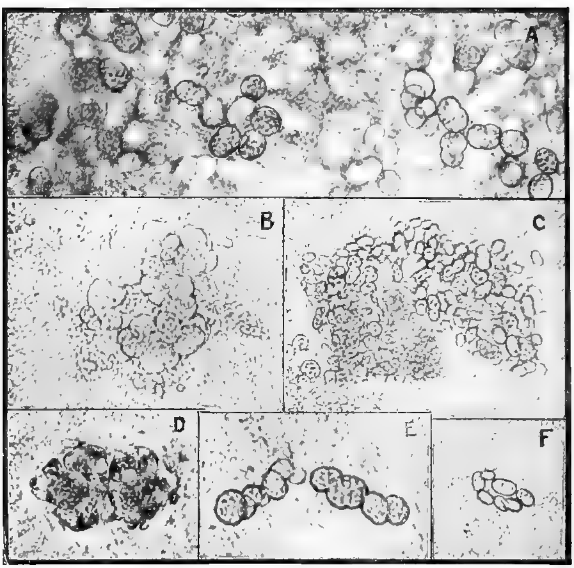

FIG. 11.

To face p. 184.

which soon pass into a resting stage, and then secrete a transparent glœal material that binds them together. The question with which we are now concerned is, *What are the microscopical changes apt to occur in such a scum or pellicle?* The scum is confessedly a composite zooglœal layer, and we are not concerned at present with the task of separating or identifying its constituent units. Our object will rather be to see in what way these units combine so as to produce new aggregates, small or large, which either singly, or after many processes of fission, result in the production of different kinds of organisms of higher type.

Before proceeding to answer this question in some detail, it is only right to point out that it is far from being the rule that the early organisms to appear in all vegetal and animal infusions are Bacteria of different kinds. In some infusions made from carrot or from turnip I have found that minute Torulæ are numerous from the first. Then, again, it is not the rule, for all infusions, that the Bacteria which accumulate at the surface should begin at once to assume a zooglœal development, as is the case with those that reach the surface of hay infusions. On the contrary, in many cases the Bacteria remain separate, for some days at least, so that no coherent membrane is formed till a later period.

Examination of one of the separate masses of Zooglœa with a high power will show its constitution, and reveal the fact that we have to do with an aggregation of separate Bacteria imbedded in a jelly-like material. The slightly altered Bacteria within the Zooglœa mass are at an early stage plainly to be seen (as in Figs. 10, A and 12, 500), though, later on, they become more or less obscured by reason of progressive molecular changes taking place in the mass during its subsequent transformation.

Some of these Zooglœa masses, as we shall see, are destined ultimately to be converted into numbers of flagellate Monads or of Amœbæ, while others become resolved into heaps of Fungus-germs. I have found it impossible to tell in their early stages from the mere microscopical appearance of the Zooglœa masses whether they are destined ultimately to yield Monads or Fungus-germs, but the latter transformation is undoubtedly the more common.

It is important to bear in mind two fairly distinct aspects of the observations about to be recorded, corresponding with different stages of the processes in question. We have to do (1) with the

growth, the individualisation, and the processes of segmentation taking place in these minute masses of Zooglœa. We have also to do (2) with the question of the ultimate destination, or the transformation, of the products of such segmentation. These are two parts of the subject that are to some extent distinct, and which are well worthy of separate consideration.

(1) *The Developmental Tendencies in Zooglœa.* If it be asked what amount of knowledge bacteriologists possess on this subject, the answer must be "extremely little," if we are to judge from the paucity of information on the subject which is to be found in any of their text-books. Yet if they would only deign to look at what takes place in the scum on a filtered hay-infusion, they could very soon satisfy themselves that Zooglœa masses not only grow rapidly, but undergo definite developmental processes, associated with marked molecular changes—as evidenced by their different behaviour at different stages to logwood or other stains, as well as by the results of microscopical examination. And, while these molecular changes are taking place, the masses may segment into larger or smaller portions and often into minute spherical or ovoid units, showing that an organising process is taking place—as may be seen in Figs. 16–20, and in others.

If we look at the constitution of Zooglœa masses, as shown in Figs. 12 and 21, A, it may be seen that we have only to do with Bacteria imbedded in a varying proportion of glœal material. But later on, when the mass has grown, and some amount of segmentation has taken place, as in Fig. 11, A, it may often be seen that we have still only to do with aggregates of Bacteria. At other times, it is true, the molecular changes that have taken place in the mass have so altered its constitution (making the segments very refractive) that the included Bacteria are no longer recognisable, as in Figs. 19 and in 21 C.

But now an important link in the proof of my views may be mentioned. The Zooglœa masses in their early stages are colourless, but a large proportion of them are, as I maintain, ultimately destined to give rise to brown Fungus-germs. The assumption of the brown colour may, however, be taken on by the segments of the Zooglœa while they are still only aggregates of Bacteria, as may be seen by Fig. 11, A ($\times 500$), showing nearly spherical segments of a large Zooglœa mass which at the time they were photographed were in different shades of brown, and in some of

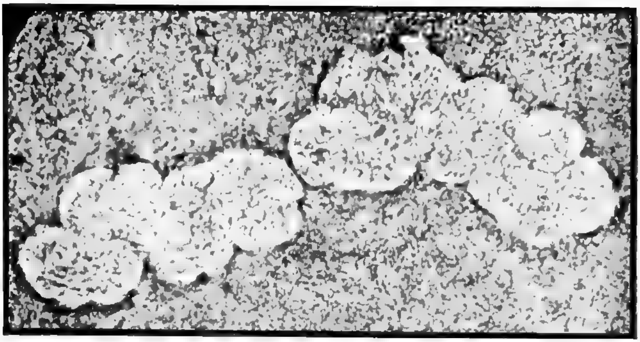

FIG. 12.

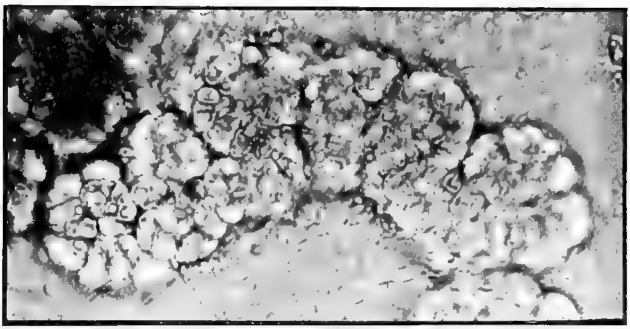

FIG. 13.

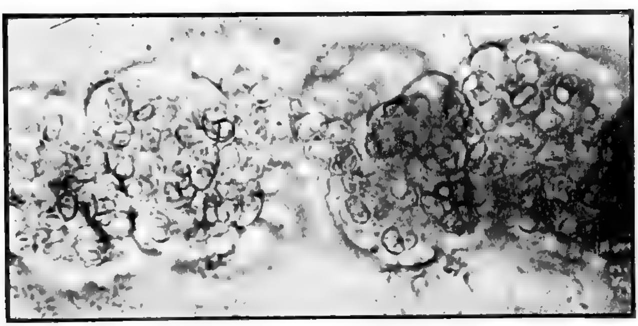

FIG. 14.

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which the constituent Bacteria (now themselves coloured) are plainly to be seen. The same thing occurs not unfrequently in Zooglœa masses which have as yet undergone little or no segmentation, as in Fig. 11, D; while in B a small segmenting Zooglœa aggregate is seen becoming brown, the process being rather more advanced below than above, while in the latter situation the mass seems to fuse almost imperceptibly with the contiguous portion of the pellicle. In C a similar mass is to be seen which has almost completely segmented into slightly brown Fungus-germs. In E we have two other much smaller masses of Zooglœa which have become brown, and which plainly show their constituent Bacteria; while in F a similar process is taking place in very minute units of Zooglœa, such as give rise to discrete Fungus-germs.

These facts concerning the changes taking place in large or small masses of Zooglœa, even leaving aside the question of their subsequent transformations, are facts of considerable importance well worthy of a little attention on the part of bacteriologists.

My more complete and recent observations as to the assumption of a brown colour by the Zooglœa masses and their segments serve to make the relationship between them and the brown Fungus-germs more obvious than it was previously. Moreover, the actual proof of the development of the one from the other can now be given in a very complete way—also as a result of recent observations.

(2) *The Transformation of Zooglœa Masses into Fungus-germs or Monads.* The very distinct specimens, to which I shall first refer were taken from the scum on a hay-infusion that had been exposed in a small closed pot to a temperature of 70° F. (21° C.) for seven days. Many of the Zooglœa masses had, by that time, become wholly transformed into brown Fungus-germs, though other masses in all intermediate stages were to be seen. Some were still in an early colourless state, as in Fig. 12. A later stage in which the whole mass is being converted into embryo Fungus-germs is shown in Fig. 13, parts of which are assuming a faint brown tinge; and a still later stage, in which the nucleated embryo germs are more distinct, and in which portions of the mass have assumed a still darker brown colour, is shown in Fig. 14 (× 700). Some of the brown Fungus-germs into which such a mass becomes resolved may be seen in Fig. 15 (× 500), beginning to grow into short chains, preparatory to the formation of mycelia.

In the pot from which these specimens were taken, at the end of the seventh day, not a single hypha was to be found from which such germs could have been derived, and all such bodies were similarly absent for many days after, during which, from time to time, I examined the contents of the pot. It was perfectly plain, indeed, that in the thousands of small Zooglœa masses undergoing this change, one had to do with no process of infection. It was clear that the Zooglœa masses were becoming organised simultaneously and throughout their whole substance, and that all the stages of the development of the Fungus-germs, into which they were being transformed, could be more or less plainly traced—the changes in this case taking place without any antecedent minute segmentation of the masses, such as are so common, and which may be seen in a small scale in Fig. 11, C, and in larger masses in Figs. 16 and 17.

A similar transformation of the Zooglœa masses without antecedent segmentation has been seen in other cases, though in none of them have I been able to make out the actual stages of the change anything like so plainly as in the specimens represented in Figs. 12 and 13. In one of the cases recently seen, in which the specimens were also taken from a closed pot, the Zooglœa masses as a whole had previously assumed a pale brown tint, and rather large Fungus-germs were formed from their substance. But, again, no hypha of any kind was ever seen among the contents taken from this pot.

Where a certain amount of segmentation has occurred in the Zooglœa mass and the brown colour is subsequently assumed (as in Fig. 11, A, in which the constituent bacteria are still distinct), the rather large brown segments subsequently become resolved into groups of small brown Fungus-germs, such as are shown in Fig. 15 (× 500).

At other times, segmentation goes on to the production of the ultimate units that are to be formed from the Zooglœa mass before any change in colour occurs. This is shown in Figs. 16 and 17 (each × 500). In the first of the two, segmentation is seen to be progressing in a colourless mass; while in Fig. 17, and especially in the upper half, colourless ultimate segments are separating and becoming brown. In the lower half of the figure, larger masses are separating and becoming brown, as in Fig. 11, A, which will subsequently divide into from two to five ultimate segments. The same kind of thing occurred in the specimen shown in Fig. 18 (× 500), which was taken on the twelfth day from a pellicle on another

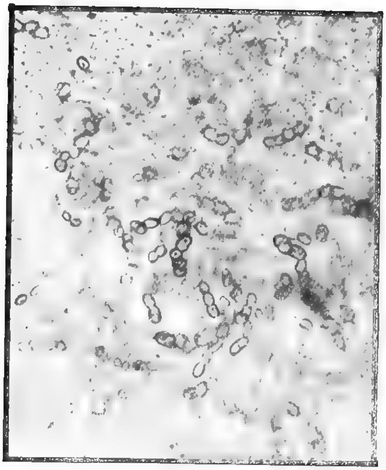

FIG. 15.

FIG. 16.

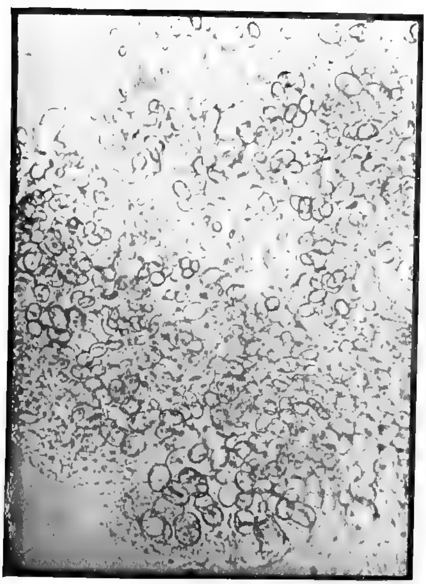

FIG. 17.

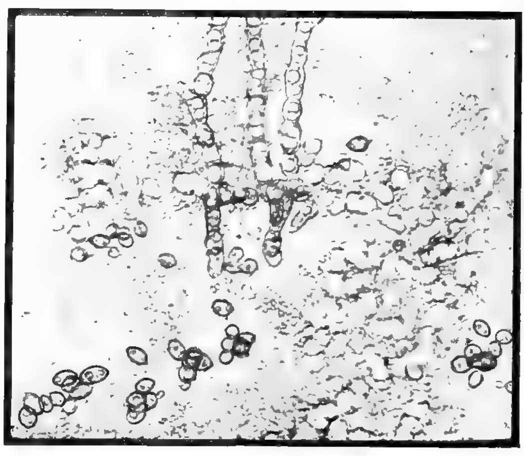

FIG. 18.

To face p. 188.

hay infusion. On the right side the principal mass of colourless segments may be seen, and some of these took on a brownish-black colour before they began to germinate, as shown in the figure. The heterogenetic Fungus-germs, thus produced, are minute ovoid bodies with one, or sometimes two, nuclear particles, such as may be seen in this case and also in Fig. 11, C and in Fig. 15.[1]

From what has already been said, it will be seen that there are various modes in which Fungus-germs originate from the Zooglœa masses. Still, these different modes are only comparatively unimportant variations, dependent upon the different states of the Zooglœa masses, in regard to colour and degree of segmentation, at the time when the actual transformations take place. The principal variations seem to be these :—

(*a*) Zooglœa entire and uncoloured; organisation throughout, and Fungus-germs, when forming, becoming brown, as in Figs. 12–14.

(*b*) Zooglœa entire, but assuming a brown colour before the transformation into fungus-germs occurs, as in Fig. 11 D, and as shown in Fig. 4 A, in "The Annals and Magazine of Natural History" for February, 1905.

(*c*) Zooglœa uncoloured, undergoing partial segmentation, and then the origination therefrom of Fungus-germs gradually becoming brown, as in Figs. 16 and 17.

(*d*) Zooglœa uncoloured and undergoing partial segmentation; these segments then becoming brown, and subsequently being transformed into groups of brown Fungus-germs, as in Fig. 11, A.

(*e*) A mixture of the last two processes, as shown to some extent in the lower part of Fig. 17.

(*f*) Zooglœa uncoloured and undergoing complete segmentation into colourless units, which gradually assume a brown or brownish-black tint, as in Fig. 18.

As previously stated, the conversion of the Zooglœa masses into aggregates of Monads takes place less frequently than their transformation into Fungus-germs. Still the two processes may often be seen going on side by side in the pellicle. I am, indeed, disposed to think from what I have seen that some of the same kind of Zooglœa masses which in the early days become converted into Monads may, if they chance to remain untransformed for seven

[1] The share taken by the glœal material in the production of these bodies is referred to in my "Studies in Heterogenesis," p. 82; as well as the relations of this glœal substance to protoplasm.

or eight days, be then converted into Fungus-germs rather than into Monads. It is difficult to be certain of this, but I am strongly disposed to believe that it is so. It is quite certain, however, that conversion into Monads, when it occurs, takes place almost always somewhere between the third and the fifth days, while after the latter date, up to the tenth or twelfth day, one finds Zooglœa masses either all brown or producing brown segments which are being converted into Fungus-germs. *But the Zooglœa masses, and all the segments into which they divide, invariably remain colourless where Monads or Amœbæ are to be the products.*

Since my recent communication to "Nature" (Nov. 24, 1904) I have seen numbers of the Zooglœa masses yielding Monads in the pellicle on an infusion of hay, which was exposed to light in a small beaker. The Monads were met with on the fourth and fifth days, the temperature, to which the infusion had been exposed, having varied from about 54°–59° F. (12°–15° C.). The Zooglœa masses were in many cases very large and presented some peculiar characters. In their early stages they always appeared as very pellucid, somewhat ramified, discontinuous areas, such as are shown in Fig. 19 (× 500); A represents an early, and B a later stage in which separation of ultimate segments is commencing. Still later stages are shown in Fig. 20 (500): portions of the mass beginning to segment may be seen on the left, while above and to the right, a more complete segmentation into motionless, colourless units is seen, which after a time may become either active, flagellate Monads, or Amœbæ—sometimes the one and sometimes the other of these convertible forms of life. A similar process taking place in other small Zooglœa masses, is plainly shown in Fig. 21 (× 500): thus in A we have small masses of Zooglœa in which the constituent Bacteria are plainly seen; in B other masses becoming more refractive and about to segment; while in C segmentation is actually occurring, and the products are still more refractive. They, as well as the products in Fig. 20, are destined to develop either into Monads such as are shown in Fig. 24, E (× 500) or else into minute Amœbæ of the same size.

Since the last two paragraphs were written I have made some observations, very interesting in many respects, but especially so from the point of view of the existence of an interchangeable relation between Monads and Amœbæ on the one hand, and Fungus-germs on the other, to which I have previously referred. These observations were of the following nature.

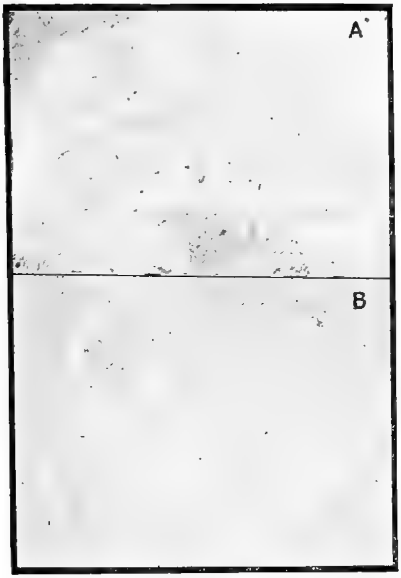

FIG. 19.

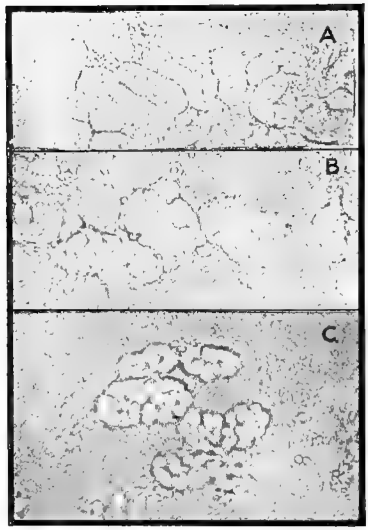

FIG. 21.

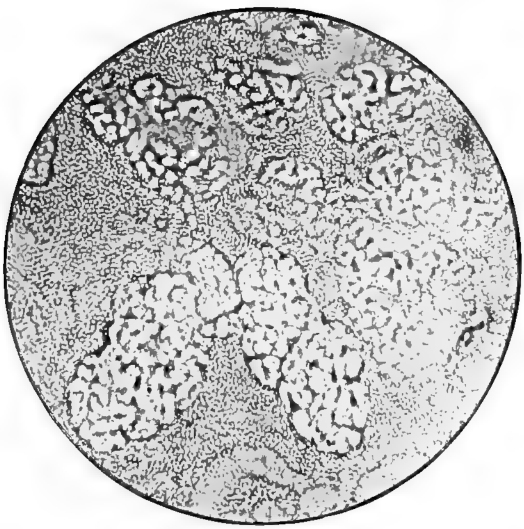

FIG. 20.

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Some specimens of Cock's Foot Grass *(Dactylis glomerata)*, just as they had begun to flower, were gathered on June 14th. On reaching home they were laid on a newspaper and placed within a drawer, where they remained till July 10th when an infusion was made from small segments of the stalks and leaves. The infusion was prepared with distilled water at 80° F. (27° C.) for three and a half hours, when one portion of it was filtered in the usual way into a small one-ounce beaker, and another small portion into a watch-glass. The latter was covered by an inverted wine-glass, and the former placed under a small bell-jar, by its side, and the temperature to which they were exposed for the next few days was about 73° F. (22° C.).

The fluid rapidly became turbid in each, and on examination, at the expiration of 44 hours, of the pellicle that had formed in the watch-glass I found it to be almost wholly composed of minute Zooglœa masses, such as may be seen in the specimen represented in Fig. 22, A (× 150). By the next day many of these small masses showed the first stage of change, two of which may be seen in B (× 500), close together. They were a trifle paler than the others, while their contained Bacteria were rather less closely and regularly aggregated. On the following day (fourth), in many of these altered, paler areas, minute brown Fungus-germs of different sizes were seen to be taking origin, such as are shown in C (× 500). The minute brown bodies were found only in these paler and altered Zooglœa aggregates; while on the following day groups of such aggregates, partly fused, were becoming brown here and there, and being converted into masses of minute Fungus-germs, as shown in D (× 500). While many of the small Zooglœa masses remained stationary for several days in this scum which formed on the shallow fluid in the watch-glass, those that underwent change produced only such small brown Fungus-germs; which grew and speedily multiplied, so as to form dense aggregates of small brown germs.

Turning now to the portion of the same infusion in the small beaker, in which the fluid had a depth of nearly two inches, its scum when first examined, after 44 hours, was found to be almost exactly similar to that shown in Fig. 22, A, from the watch-glass. There was the same close aggregation of small Zooglœa masses, though the scum itself was rather thicker than that on the fluid in the watch-glass. Two days later (fourth day) many of the masses were larger, and were segmenting into colourless units exactly like those which commonly, at this early stage, develop into Monads or

Amœbæ. This was the only kind of change seen, and is well shown in different stages in Fig. 23 (× 500), together with some of the unchanged masses below and to the left.

For some reason these colourless units, exactly similar in appearance to those of Fig. 20 which straightway developed into active Monads, remained without further change during the fifth, sixth, and seventh days; then, on the eighth day, in a few of these masses the colourless units were seen to be developing into brown Fungus-germs, while, on the ninth day, very many of them were undergoing this change—and still not a single Monad or Amœbæ was to be found. It looks, therefore, as if some change in the chemical constitution of the fluid prevented the development of the segments into Monads or Amœbæ (as is so common during the third or fourth days), and yet after a time favoured their development into brown Fungus-germs.

(3) *On the Production in and from the Pellicle of discrete Corpuscles which speedily develop into Flagellate Monads, or Amœbæ, or else into Fungus-germs.* At the expiration of three or four days, according as the temperature to which the infusion is being exposed is up to, or some degrees below, 70° F., the first indication of the formation of discrete corpuscles may be found. When a portion of the pellicle has been taken up on the tip of a sterilised scalpel, and rotated off on to a drop of water or of some staining fluid placed on a microscope slip, and when the cover-glass has been applied, we find on examination a thin membranous, granular-looking layer surrounded by myriads of free and active Bacteria—though no free Monads or Amœbæ are to be seen. This layer will probably be already flecked by many of the minute Zooglœa masses already described; but on examination with a power of four or five hundred diameters portions of the pellicle lying between them may be seen to display a more or less evenly granular appearance, owing to the Bacteria, entering into its formation, having in part ranged themselves at right angles to the surface of the fluid, and being motionless by reason of being imbedded in the viscid glœal material which they have excreted.[1] In many other places, however, especially when the pellicle is looked at slightly above

[1] Though motionless, they are far from being dead as Pouchet imagined. He said (" Hétérogénie," 1859, p. 354) : " La pellicule proligère étant constamment formée par les cadavres des animalcules dont les générations se sont succedé." He, in fact, invariably spoke of the Bacteria in the pellicle as "les cadavres."

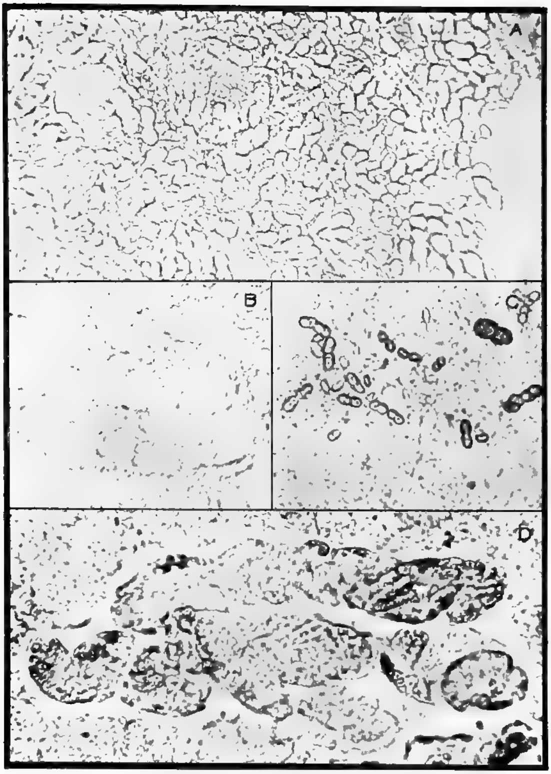

FIG. 22.

FIG. 23.

To face p. 192.

the proper focal distance, appearances such as are represented in Fig. 24, B, C (× 500), are to be observed. That is, the pellicle in these situations is seen to be pretty closely packed with a number of minute, motionless, and rather ill-defined whitish corpuscles, which are almost always found to be located in the under layer of the pellicle.

I have over and over again noticed these appearances in the pellicle, and even more distinctly as in Fig. 22, D, owing to the corpuscles being more fully formed, when there was not a single active Monad or Amœba in or around the portions under examination. But when other portions of the same pellicle have been examined, twelve to sixteen hours later, thousands of active Monads have been present, all of about the same size as the motionless corpuscles previously seen to be forming, and each moving more or less rapidly by means of a single flagellum. These active Monads, such as are shown in Fig. 22, E (× 500), are to be found, not only around the portion of the pellicle under examination, but beneath it where previously only motionless corpuscles existed.

For a long time, owing to the discrete corpuscles in their motionless condition being situated on the under surface of the pellicle, I found it very difficult thoroughly to satisfy myself as to their actual mode of origin. The general indications were certainly strongly in favour of their having been formed in and from the pellicle itself, as may be gathered from the following considerations :—

(1) Thousands of Monads appear comparatively suddenly of full size, and often when their appearance has been seen to be preceded by the gradual formation of multitudes of motionless corpuscles of the same size in the under layers of the pellicle.

(2) Never in their motionless condition, and only very rarely in their free active state, was any evidence of multiplication by fission to be seen, although many thousands of these Monads have been examined from time to time.

(3) In the course of five or six days, when the weather is warm, Monads are in some pellicles formed in enormous numbers, and obviously at the expense of the pellicle itself, since whole regions of this membrane actually disappear where they have been formed, leaving only intervening ridges in the midst of which other motionless corpuscles appear to be forming.[1]

[1] This is shown in "Studies in Heterogenesis," Pl. VI., Fig. 54, D.

These facts and considerations alone, as I have said, seemed to indicate very forcibly that the corpuscles were formed from the very substance of the pellicle, and that they had not sprung up from multitudes of invisible germs, gradually growing till their full size had been attained, and then multiplying with great rapidity.

After a time, however, I thoroughly satisfied myself that the corpuscles are, as a matter of fact, only individualised portions of the general Zooglœa mass of which the pellicle is composed—each corpuscle containing several Bacteria. And often when the corpuscles separate, and first begin to exhibit slow oscillating movements, they may be seen to have an exactly similar composition. They appear then as small pellucid spheres, having a single flagellum, and containing in their interior four or five Bacteria, exactly like those in the region of the pellicle from which they have been derived.[1] These Monads soon increase somewhat in size and become more active ; the Bacteria in their interior are no longer distinguishable and a distinct nucleus is formed, as in Fig. 24, E.

In some pellicles the discrete corpuscles that are produced in the manner indicated develop into Amœbæ rather than into Monads. This probably depends upon some differences, of an unknown nature, in the chemical constituents of the infusion itself favourable to the production of this phase of the Monad-Amœba couple. That the two forms are rapidly and easily convertible, the one into the other, is very generally admitted. In an infusion made from a bunch of *Melica nutans* in full flower, minute Amœbæ were produced in myriads from the pellicles in this way. Fig. 26, A ($\times$ 375) shows a number of these Amœbæ varying much in size, which were found in the pellicle on this infusion. Again, in an infusion made from mixed grasses in full flower (largely composed of *Lolium perenne*) a thin pellicle was observed, the weather being warm, in which Monads were scarce, but vast numbers of small active Amœbæ existed as early as the second day. In regard to the appearance of this pellicle on the fifth day my note-book says : "Three-fourths of the pellicle is now apparently converted into Amœbæ, which exist in myriads. Not a single

[1] Sometimes, however, the Bacteria are not so clearly recognisable, and the corpuscle as a whole is much more refractive. Similar differences exist also, as I have shown, in regard to the segments into which embryonal areas divide.

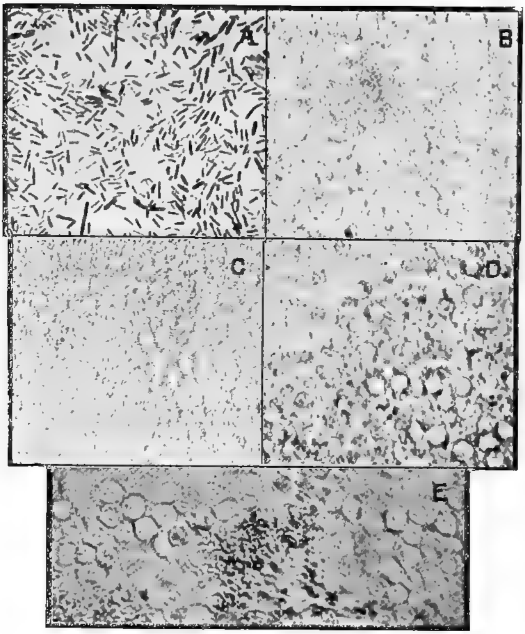

Fig. 24.

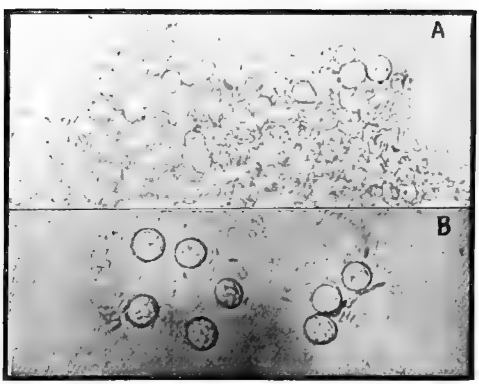

Fig. 25.

To face p. 194.

one of them has been seen to divide." In some ordinary hay infusions also, Amœbæ are produced in great numbers from the discrete corpuscles. A number of small and large Amœbæ that were found with multitudes of others on the surface of a rather old pellicle, in which they had had time to grow, are shown in Fig. 26, C (× 500); while B represents other Amœbæ beginning to encyst in another hay pellicle on the twelfth day, the upper one of which still exhibited very sluggish movements; and D (× 500) shows many others completely encysted, in which the protoplasm is, in some of them, more or less contracted, and shrunk away from the cyst-wall.

An infusion of roses made from the flowers, leaves and stems, or even from the petals only, often yields Amœbæ in great abundance. In making an infusion from the latter, the petals were allowed to macerate for about twelve hours before the fluid was filtered off. The Amœbæ found in such infusions are sometimes very short-lived. Thus, in one of them they were very abundant and active on the fourth day, but two days later they were almost all found to have become encysted.

The production of Amœbæ in abundance is again practically invariable in mixtures of egg and water. When one tea-spoonful of mixed white and yolk of an egg is added to eight ounces of filtered water, enormous quantities of Amœbæ are to be found, after a time, in the pellicle which forms on such a mixture, though they have generally been preceded by the appearance of Monads. A very thick and glistening pellicle is commonly produced, and after six or seven days Amœbæ appear therein in great abundance. Their presence may be rendered most obvious by allowing portions of the pellicle to soak in a drop or two of logwood solution for some hours. Their actual mode of origin is apt to escape observation owing to the thickness of the pellicle in which they are formed. Still, on a few occasions, I have been fortunate enough to find very cogent evidence that they are formed in just the same way that the discrete corpuscles are produced from the pellicles on hay infusions—that is, by individualisation and metamorphosis of minute portions of Zooglœa.

As these discrete corpuscles sometimes develop into Monads and sometimes into Amœbæ, I have been accustomed to speak of them as "indifferent corpuscles." Not unfrequently, however, after their formation, they may remain for a long time quiescent and without developing in either direction. This was the case in a hay

pellicle that I have recently examined, in which such corpuscles were found in enormous numbers, many of them being rather larger than usual. They were first seen when a small pot containing some hay-infusion was opened after four days, during which it had been exposed to a temperature of 70° F. (21° C.). The edge of a portion of this pellicle is represented in Fig. 25, A (× 500), while in B (× 700) some of the separate corpuscles are shown more highly magnified, so as to reveal the nature of their contents. Nothing like a nucleus is to be seen, nor can one be detected by the use of any of the ordinary stains, even when the corpuscles have been allowed to soak in them for many hours. Logwood, carbo-fuchsine, gentian violet, and mastzellen stain have all yielded negative results. Not a trace of a nucleus is to be found, and the corpuscles seem to be mere individualised portions of Zooglœa intermediate in size between the brown units on the way to the production of Fungus-germs, which are shown in Fig. 11, E and F, and, like them, containing only a few Bacteria in their interior. I have examined such corpuscles over and over again, and always with similar results. If they do not speedily develop, a limiting membrane is produced which enables their contents to resist staining for some time, and causes the corpuscles themselves to shrivel if they are mounted in glycerine and water. When, however, these corpuscles develop quickly, which is the rule, they give rise more or less suddenly, as I have said, to swarms either of Monads or of minute Amœbæ, and then, when thus developed, a delicate nucleus can generally be recognised even without the aid of stains.

While the discrete corpuscles that develop into Monads and Amœbæ seem almost always to be produced from the under layer of the pellicle, those that develop into Fungus-germs are almost as invariably produced from its superficial layer. The fact of their origin from the substance of the pellicle itself is therefore, as a rule, much more readily to be made out than that of the discrete corpuscles whose origin and nature we have previously been considering.

The discrete Fungus-germs may easily be observed to commence by the individualisation of small ovoidal or spherical portions of the pellicle, in which, at first, the contained individual Bacteria are distinct, as may be seen in Fig. 27, A (× 375), which shows the germs as they were originating on the third day on the surface of a hay pellicle; or in B, on the fifth day, from a pellicle on an infusion

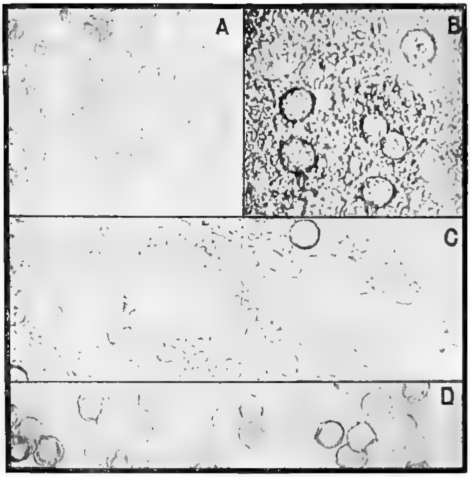

FIG. 26.

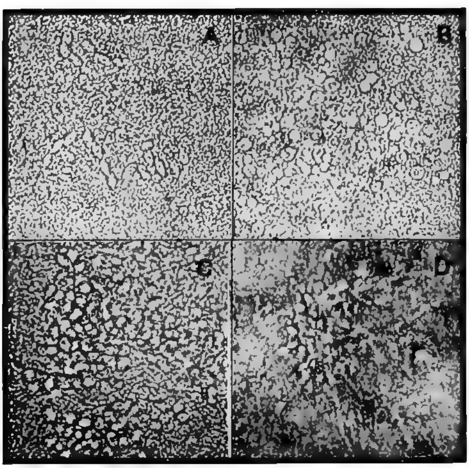

FIG. 27.

To face p. 196.

of rye grass (*Lolium*) ; and again in C from another infusion of the same grass which was made more than twelve months later.[1] In D similar corpuscles, taken from a pellicle on a carrot infusion on the third day, are developing mycelial filaments.

It not unfrequently happens that the units which originate in this way become, as they develop, more refractive, as shown in C and D. More frequently, however, they may notably change in colour—becoming brown, or even brownish-black, as they mature. This latter change is seen in Fig. 11, E and F.

Of course, all that has been said in this chapter is very contrary to generally accepted beliefs ; but if others would only investigate the subject with care, as I have done, there can be little doubt but that my results would speedily be confirmed. When will the bacteriologists tell us what they know about Zooglœa—whether they are, or are not, aware of its developmental tendencies, and why it should undergo processes of minute segmentation, unless such processes are a result of an organising tendency destined to have some definite outcome ? How can the production of Monads and Amœbæ from its colourless segments be explained other than I have done ? Why, again, should it, or its segments, so often tend to assume a brown colour, while it is still nothing but Zooglœa, either segmented or unsegmented ? Again, why, if the brown Zooglœa does not yield the brown Fungus-germs, should there be this constant association of myriads of brown Fungus-germs (in the absence of hyphæ) in association with brown masses of Zooglœa ? How can they explain, other than I have done, the actual organisation of a Zooglœa mass and the stages by which the brown Fungus-germs seem to be formed therein, such as are shown in Figs. 12, 13 and 14 ? What process of "infection," in a filtered hay-infusion contained in a closed pot, could cause thousands of small Zooglœa masses to go simultaneously through similar processes of this kind —producing myriads of brown Fungus-germs—when not a single hypha is anywhere to be found, and where, at first, no Fungus-germs are to be met with outside the Zooglœa masses themselves ?

This production of different kinds of living organisms, by the fusion and differentiation of aggregates of simpler living units, is something altogether unique, and hitherto unknown to biological

[1] The grass in the interval having been kept in a small cardboard box.

science. It suggests however now, as it did in 1872, a striking analogy, to which I then referred in the following terms[1]:—

"Just as we have supposed that living matter itself comes into being by virtue of combinations and rearrangements taking place amongst invisible colloidal molecules, so now does the study of the changes in the 'pellicle' absolutely demonstrate the fact that the visible, new-born units of living matter behave in the manner which we have attributed to the invisible colloidal molecules. The living units combine, they undergo molecular rearrangements, and the result of such a process of Heterogenetic Biocrasis is the appearance of larger and more complex organisms; just as the result of the combination and rearrangement between the colloidal molecules was the appearance of primordial aggregates of living matter. Living matter is formed, therefore, after a process which is essentially similar to the mode by which higher organisms are derived from lower organisms in the pellicle on an organic infusion. All the steps in the latter process can be watched; it is one of synthesis—a merging of lower individualities into a higher individuality. And although such a process has been previously almost ignored in the world of living matter, it is no less real than when it takes place amongst the simpler elements of not-living matter. In both cases the phenomena are essentially dependent upon the 'properties' or 'inherent tendencies' of the matter which displays them."

[1] "The Beginnings of Life," vol. ii, p. 262.

CHAPTER XI

SOME MISCELLANEOUS EXAMPLES OF HETEROGENESIS

(a) On the Relations between certain Diatoms and the Fission Products of a parasitic Alga (Chlorochytrium).

MUCH interest was excited in 1872 owing to the discovery by F. Cohn[1] of an Alga existing, as a parasite, in the thallus of the ivy-leaf Duckweed (*Lemna trisulca*). This was followed in 1877 by the discovery of another parasitic Alga by Prof. Perceval Wright[2] infesting various marine Algæ. Since this time, several other forms have been discovered, and rather an extensive literature has grown up concerning Chlorochytrium and allied genera.

Among the new forms there is one *Ch. Knyanum*, found in *Lemna gibba* and in *L. minor*, which was examined and figured by G. Klebs[3] in 1881. This is evidently the Alga that I have of late met with very abundantly in both these species of Duckweed, and to which my present remarks will refer.

I found, during the autumn and winter of 1902, among Duckweed from various localities, many dead and decolourised leaves, having a greyish-white and somewhat gelatinous appearance. Such leaves may be easily picked out, by spreading some of the Duckweed in a thin stratum of water over a white dish. It will then be found that the decolourised leaves are all devoid of rootlets, and possibly this loss of the rootlets may have been the main cause leading to the premature death and to the change in the appearance of the leaves.

Examination with a hand lens, magnifying eight or ten diameters, will show, in many of such leaves, that the upper greyish-white surface is flecked with minute specks of an emerald-green colour,[4]

[1] "Beiträge zur Biologie der Pflanzen," Heft ii., p. 87.

[2] "Trans. Roy. Irish Acad.," vol. xxv., p. 13.

[3] "Botan. Zeitung," 1881, 248, t. iii., p. 11–15.

[4] The changes, now to be described, were found to be very much less common in 1903 and 1904.

sometimes abundantly and sometimes sparsely : while examination of these or other leaves under the microscope will often show an abundance of the early stages of such bright green specks, so minute as to have been invisible with the mere hand lens.

It is best to pick out the smaller leaves for microscopical examination, and even then (especially with *L. gibba*) the examination can often only be satisfactorily carried out by placing one of the leaves in a drop of water on an excavated glass slip (taking care that its upper surface is uppermost) and gently compressing the leaf, if necessary, with the cover-glass.

An examination of a very large number of these infected leaves has enabled me to ascertain the following facts.

The very active spores of the Chlorochytrium penetrate to some of the intercellular spaces of the leaf through the stomata. Single spores, or such bodies after a primary fission, may be seen just within the stomata. Sometimes the entire spore, or the segments of the once or twice divided spore, will grow considerably before undergoing any further fission though, more commonly, division goes on so as to produce eight or more cells which, as they grow, soon become tightly packed within the now dilated sub-stomatal space. Examination of the surface of the leaf over one of these patches will always reveal a stoma greatly dilated and almost circular in shape.

The mode of infection in *L. minor* and *L. gibba* is, therefore, altogether different from that described by Cohn as occurring in *L. trisulca.* In that species of Duckweed there is curiously enough an absence of stomata. The average shape and appearance of the patches of Chlorochytrium in it is also rather different from that of the patches in the other two Duckweeds ; and the patches in the latter likewise lack the distinct, and often thick, bounding membrane which occurs round the patches in *L. trisulca.*

In each of the forms the tendency is to an ultimate production of minute spherical or ovoidal zoospores, which, after exhibiting a swarming movement, may make their way out of the space where they have been developed. It often happens, however, in each of these forms of Chlorochytrium, that the zoospores may, either in whole or in part, not succeed in escaping, but come to rest within their respective cells or spaces.

Multitudes of partially empty spaces may be seen containing large or small specimens of the intermediate fission products, those within the same space being either all of one size (Fig. 28, C,

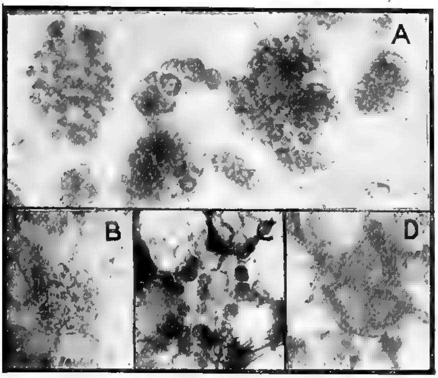

FIG. 28.

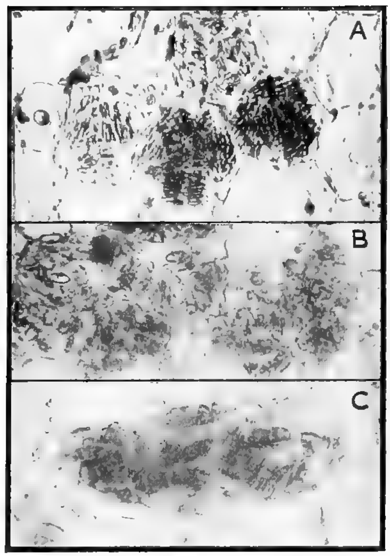

FIG. 30.

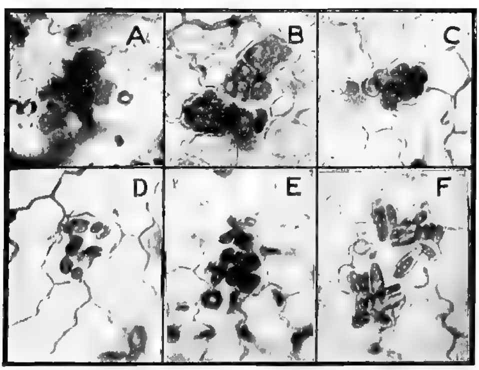

FIG. 29.

To face p. 200.

× 250), or of very different sizes. Other spaces may be seen still full and distended with Chlorochytrium, the constituent cells of which exhibit very different degrees of segmentation, as in Fig. 28, A. Some have become resolved into the very minute zoospores as in B (× 375), while others have remained as fission products varying much in size.

Thus the Chlorochytrium cells are found to undergo processes of division to a variable extent so as to yield fission products of very different sizes; and, presumably under the influence of some unfavourable conditions in their environment, some of the products, at each of these stages, may undergo no further changes of a normal kind.

This brings me to one of the important points now to be made known—which is, that in the later stages of the life of *Ch. Knyanum* the fission products, within the intercellular spaces of the leaf, are often found to be more or less intermixed with Diatoms, varying much both in size and shape.

This association is met with sometimes in spaces none of the contents of which have escaped, and then the contrast is great between the beautiful emerald-green of the algoid cells and the brownish-yellow colour of the Diatoms mixed therewith. At other times partially empty spaces are seen containing the fission products of the Alga alone (Fig. 28, C), Diatoms alone as in D (× 250), or a mixture of the two kinds of units.

More rarely, spaces are found densely packed with brownish-yellow Diatoms only, in different stages of growth and development, except perhaps for the association of one or two minute algoid corpuscles.

In regard to the Diatoms themselves, they are sometimes very small and rudimentary, or they are much larger; and these larger sizes are either fairly broad and ovoid like Naviculæ, or else narrow and elongated, like Nitzschiæ.

In almost all cases, however, the Diatoms have the appearance of being immature; they have ill-developed siliceous envelopes, and are all quite full of brownish-yellow endochrome. There are also, at times, indications that growth and multiplication of these immature forms is, or has been, taking place—looking to the way in which they are occasionally ranged side by side in short rows in some of the half-empty spaces.

The sub-stomatal spaces which have been tenanted by the Chloro-

chytrium are characterised, as I have said, by a greatly distended and almost circular stoma, and often by having their walls stained of a more or less distinct rust colour. Indications of the latter change can be seen in Fig. 26, C, D.[1] It is a fact of much importance that Diatoms are never to be found in any of the sub-stomatal spaces except in those which either actually contain, or bear marks of having been previously tenanted by, Chlorochytrium.

There is another point of much interest to be mentioned.

Sometimes one of the epidermal cells, of zig-zag outline, will here and there be found filled by a light green Alga, having the appearance of being a species of Chlorochytrium, as in Fig. 29, A (× 375). Other of these cells may be found in which such bodies seem about to undergo fission into several smaller cells, as in B; and others still in which the original cell has divided into small green ovoid products (C), or into a number of more minute zoospores. In one case such zoospores were seen to have assumed a yellow colour, and some of them seemed to be elongating, as was the case with some of the segments shown in D. Many other of these isolated epidermal cells have been found containing either small ovoid Diatoms only (Fig. 29, F), or a mixture of such Diatoms with green fission products, as in E; just as I have found the two kinds of bodies associated in the much larger sub-stomatal spaces.

The Diatoms in the epidermal cells are always small, commonly of about the same size, but not invariably so, and mostly have the appearance of being minute Naviculæ.

How the Chlorochytrium spores obtain an entry into these epidermal cells I am unable to state; but being actively motile it would clearly be much easier for them to get in, than for the Diatoms to do so.

It seems most probable that it is the spores of *Ch. Knyanum* which infect these epidermal cells, and that they may penetrate them from a sub-stomatal space, since I have often, though by no means invariably, found such infected epidermal cells just over, or by the side of, one of these spaces.

What interpretation is to be given concerning the Association of the Diatoms with the Chlorochytrium fission products?

Only two possibilities seem to present themselves :—

[1] Of course these two characteristics, belonging to different planes, can never be seen together in the same photograph.

(*a*) The Diatoms have, like the Algæ, obtained entry to the sub-epidermal spaces through the stomata.

(*b*) The Diatoms have been produced *in situ* by a transformation of the fission products of the Algæ.

The first of these possibilities it will be convenient to speak of as the Infection Hypothesis, and the second as the Transformation Hypothesis.

(*a*) *Infection Hypothesis.* The difficulty in accounting for the facts seem to me to be extreme in accordance with this supposition, especially if we bear in mind what is authoritatively known concerning Diatoms. The important points are these :—

1. No *motile* spores are known ; and previous to 1896 there was no certain knowledge concerning the existence of spores of any kind in Diatoms. The important discovery by Geo. Murray of undoubted spores or germs, originating by a process of rejuvenescence, in species belonging to three marine genera, constitutes all that is certainly known at present on this subject.[1]

2. It is commonly stated by writers that individual Diatoms do not increase in size[2]; increase in bulk of Diatoms being only brought about as a result of conjugation, which is admitted to be a comparatively rare process.

3. Previous to the above-mentioned discovery by Geo. Murray, Diatoms were said to be formed only (1) by a process of conjugation, or (2) by fission—the latter being the common process, and one that is generally said to involve a very slight diminution in size of the products.[3]

Such facts, concerning Diatoms in general, must be borne in mind in conjunction with these others, more especially bearing upon the question now under consideration.

4. The sub-stomatal spaces that either are, or have been, tenanted by Chlorochytrium probably constitute much less than ten per cent. of those existing on most leaves of the Duckweed, yet no Diatoms are ever to be seen in the other ninety per cent. of the sub-stomatal spaces.

5. The purposeless to and fro movements of some Diatoms when free in a fluid, and their absence of movement when lying on the surface of a leaf, seem quite incompatible with the notion

[1] "Proceed. of Roy. Soc. of Edin.," 1896–7, p. 207.

[2] Wolle, "Diatomaceæ of North America," 1890, p. 11.

[3] Smith's "British Diatomaceæ," vol. i., 1853, p. xxiv., and vol. ii., 1856, p. vii.; and Pritchard, "History of the Infusoria," fourth edition, 1861, pp. 58, 61–63.

of their selective penetration through certain special stomata only.

6. A point of still greater importance is the fact that Diatoms are never to be seen in the spaces in which the Chlorochytrium is in one of its early stages of development; they are to be found only in association with its later stages, where some of the final segmentations have been taking place, and often where the patches are so old that the walls of the spaces containing them are stained a rust colour.

7. None of the Diatoms found either within the spaces or within their ramifications between surrounding cells have ever been seen to move.

8. Moreover, where the Diatoms exist they are often intimately intermixed with the algoid cells; they are also to be seen in the ultimate ramification of spaces, even when these are still full; and small specimens are likewise to be found between surrounding sub-epidermal cells contiguous to the invaded space. Such facts are incompatible with an entry of Diatoms from without, if we bear in mind what has been said under the last two heads.

9. Again, where the Diatoms exist they not only vary much in size and shape in different spaces, but even within different regions of the same space.

Taken as a whole, these various facts seem to me absolutely to negative the Infection Hypothesis as a means of accounting for the association of Diatoms with the fission products of Chlorochytrium in the sub-epidermal spaces.

(*b*) *Transformation Hypothesis.* The facts which are so incompatible with the foregoing hypothesis will be found to offer no difficulties to, or to be capable of receiving a ready explanation in accordance with, the transformation hypothesis. This hypothesis is also strengthened by other facts not previously referred to.

1. The absence of the Diatoms from the ninety per cent. of the sub-stomatal spaces which are not infected by the Alga is explained.

2. The absence of movements on the part of the Diatoms in question affords no difficulty.

3. The absence of the Diatoms from the Chlorochytrium spaces during the early stages of the development of the Alga affords no difficulty and is explained.

4. The variation in the size of the Diatoms is explained, in the main, by the varying size of the fission products of the Alga. The

two kinds of units very commonly co-exist, and where the Algoid cells are small the Diatoms are small; where they are of medium or larger size the Diatoms are similarly of medium or larger size. Such variations in the size of the Algoid cells are very common within the same infected space; and then, when Diatoms are present, they also are of various sizes.

5. Old, partially empty spaces are often to be seen containing the Chlorochytrium fission products, small or large; others may be found containing Diatoms, small or large; and others again partially empty, but containing a mixture of the Algoid fission products with Diatoms of a corresponding size.

6. Other spaces, still densely filled, will show, in association with the Algoid cells, Diatoms, either packed in their midst or occupying the boundaries of spaces, and often differing greatly in size in the two situations. They are likewise to be found occasionally in the narrow spaces between contiguous spherical cells, where Algoid spores from the parent brood not unfrequently penetrate.

7. In the spaces where the Algoid cells and the Diatoms are mixed, some of the cells may be seen to have assumed the brownish-yellow colour of the Diatoms; and some of such cells may also be seen more or less elongated, and apparently developing into Diatoms.

8. The majority of the Diatoms have an immature appearance. The siliceous envelope, in the great majority of them, seems to be either absent or very imperfectly developed; and unmistakable evidence that multiplication of these immature Diatoms has taken place is frequently to be seen.[1]

There is no probability, and no one, I think, is likely to maintain, that Diatoms are normal phases in the life-history of this parasitic Alga; and as a careful consideration of the evidence, as a whole,

[1] Some of the differences in size, apart from those due to differences in the size of the Algoid fission products from which the Diatoms originated, may be owing to actual increase of bulk in these immature organisms. Although this supposition is at variance with commonly received views, it is in accord with the observations of Geo. Murray, who says (*loc. cit.*, p. 216) that young Diatoms formed within a parent by a process of rejuvenescence, when liberated by "the separation of the parent valves at the girdle, may grow, divide and multiply before fully attaining the characteristic external sculpturing and adornment of the parent." Young Diatoms originating in fresh water may find silica in all pond water. The ammonia contained in rain water, like other alkalies, easily dissolves silica or aluminium silicate when in a finely pulverised state, and one or other of these compounds is to be found in all soils. (See Prof. A. M. Edwards, "On the Solubility of Silica," "The Chemical News," January, 1896, p. 13.)

appears utterly irreconcilable with the infection hypothesis, we seem unavoidably driven to the conclusion, which is so congruous with all the facts, that the Diatoms in question are heterogenetic products, actually produced by the transformation of the cells of the Alga—alike in the sub-stomatal spaces and in the epidermal cells.

I include the epidermal cells in this statement because almost all that has been said against the infection hypothesis and in favour of the transformation hypothesis, as accounting for the presence of the Diatoms in the sub-stomatal spaces, holds good also in regard to their presence in the epidermal cells. In one respect the argument is even stronger in its application to them, since there is much evidence to show that Diatoms are only found in those epidermal cells which are or have been tenanted by the Alga, and such infected cells never constitute more than the smallest fraction per cent. of those existing on the whole upper surface of a leaf.

A further point of extreme importance is to be found in the very great differences in the size and shape of the Diatoms, according as they originate from the small or the larger algoid fission products. Yet these variations, for which no other contributory cause is apparent, are so great that botanists, unaware of the origin of the Diatoms, and finding them in the Chlorochytrium spaces, would almost certainly regard some of them as belonging to different species of the same genus, and others even as representatives of distinct genera. This, however, is a subject which must be left for future investigation.

It was suggested to me by a distinguished botanist to whom I showed some of the specimens of Duckweed, containing in their sub-stomatal spaces and epithelial cells mixtures, in various proportions, of Chlorochytrium segments and Diatoms, that their association might be explained by the Infection Hypothesis, backed by the assumption that *Chemotaxis* had been in operation—which, in this case, would mean that the physico-chemical processes associated with the growth and multiplication of the Algæ within the spaces were capable of giving rise to products exercising an attractive influence upon the Diatoms.

It was not pretended that there was any direct evidence in favour of this assumption ; it was advanced as a possible explanation, and merely to stave off the conclusion, otherwise inevitable, that the Diatoms had been produced by the transformation of the cells of the Alga.

A careful and unbiassed consideration of the following facts will, however, I think, make it plain that the evidence is overwhelmingly against Chemotaxis and the Infection Hypothesis :—

1. Chemotaxis can only be supposed to operate at short distances. But such Diatoms as are found within the spaces are never to be seen on the surface of the Duckweed.

2. The Diatoms that are commonly met with on the surface of the thallus (a comparatively large Navicula and a Cocconeis) are never found within the sub-stomatal spaces or the epithelial cells.

3. Chemotaxis implies a direct power of movement in response to an attractive influence. But none of the Diatoms on the surface of the leaf, within the spaces, or within the epithelial cells have ever been seen to move.

4. The Diatoms in the spaces are found intimately intermixed with the Algoid cells, and generally in situations into which they could not be supposed to have the power of penetrating.

5. The Diatoms can often be seen to have replaced Algoid cells, rather than to have pushed them aside.

6. Finally, in places, the Algoid cells can be seen elongating into the forms of the Diatoms, and at the same time changing from a bright green to a brownish-yellow colour.

Moreover, since making these observations on *L. gibba* and *L. minor*, I have ascertained that similar transformations of some of the fission products of the Chlorochytrium which infests *L. trisulca* are also to be met with in that species of Duckweed. The Diatoms found in this species have been almost always very small and of the Navicula type—no Nitzschiæ having ever been seen in association with the segmentation products of this particular variety of Chlorochytrium, although the Duckweed bearing it has been taken from one of the same ponds from which I have obtained my supplies of *L. minor* and *L. gibba*.

It is worthy of note in this connection that in *Lemna trisulca* there are no stomata. The active algoid spores penetrate, as F. Cohn showed, by boring between the epithelial cells into subjacent spaces, where they increase and multiply in practically closed cavities, and become also surrounded by a kind of capsule. Subsequently these active spores make their way out through very minute apertures which they themselves form, but in this species of Duckweed there are no widely dilated stomata through which in earlier or in later stages, should they attempt it, Diatoms would be free to enter.

In Fig. 30 (× 375) some of the combinations that have been met with are shown. In A four small spaces are represented. In the upper one Algoid segments and Diatoms are intermixed; in the one on the left young Diatoms were seen forming—the contents of this space being distinctly paler than those of the other two spaces in which the Diatoms were more fully formed and more closely packed together. In B, two or three fused contiguous spaces are shown in which Algoid cells and Diatoms, together with various intermediate forms, were intimately intermixed; while C is the only space that I have yet found in *L. trisulca* containing Diatoms as large as are there represented. They were mixed with Chlorochytrium cells, as well as other minute Diatoms, though the latter are not recognisable in the photograph.

Another point is also of much importance, and that is, the frequency with which Diatoms may be seen *around the periphery of spaces still densely crowded with Chlorochytrium products* which have not yet begun to emerge. These Diatoms, therefore, make their appearance within closed cavities, and often in regions far removed from the original point of entry of the active Algoid spore. No infection hypothesis, even backed by a further hypothesis of chemotaxis, is, I submit, capable of explaining the presence of these Diatoms. They are evidently formed where they are found by a transformation of the Algoid cells, and different stages of the process may often be clearly recognised—the spherical cells, as I have said, becoming elongated, and changing from a bright green to a brownish-yellow colour as they take on the forms of the Diatoms.[1]

(b) On the Origin of Anabena from the Cells of Chlorochytrium lemnæ.

In the memoir by Ferdinand Cohn, "Ueber Parasitische Algen,"[2] in which this Chlorochytrium was first described as infesting *Lemna trisulca*, he figures four different kinds of organisms as occasionally to be met with in spaces that had been previously tenanted by Chlorochytrium, namely, a species of Raphidium, of Mastigothrix, of Leptothrix and of Nostoc. He assumed that these were all parasites which had, in some way,

[1] See also "Studies in Heterogenesis," p. 183, concerning the origin of Diatoms from Algoid cells of another kind which are often found infesting the different species of Duckweed.

[2] "Beiträge zur Biologie der Pflanzen," 1872, p. 97

obtained entry to the spaces in question. He did not attempt to verify this assumption; he seems to have taken it for granted, in accordance with generally received doctrines, that this was the proper explanation to be given.

I have never met with either of the first three organisms, named above, in the Chlorochytrium spaces, but I have, on many occasions, seen one of the Nostoceæ; and am prepared to adduce evidence as to its mode of origin within these cavities. I have, however, never seen the slightest evidence in proof of the assumption that they get in from without. Cohn says he found a representative of the genus Nostoc, but the organism which I have seen is, apparently, an Anabena. The filaments were free, and certainly not imbedded in any mucilage or jelly-like matter. And in reply to a query of mine Dr. M. C. Cooke writes, "filaments without the definite mucilage do not, to my mind, constitute a true and veritable Nostoc. I could only suggest Anabena, with free filaments, as an alternative to Nostoc."

It must be borne in mind, however, that the filaments in each of these genera are endowed only with a minimum amount of mobility, of an oscillating type, and that their spores are *non-motile;* so that the conditions are very unfavourable to their parasitism within closed cavities, or cavities with apertures only of the most minute dimensions.

I have found that specimens of Lemna containing these Algæ, when mounted in a mixture of glycerine and formalin[1] are well preserved, and are in some respects more favourable for examination than when they are simply immersed in water. The colours of the Nostoceæ are preserved extremely well in specimens thus mounted.

Just the same kind of combinations are to be met with as in the case of the transformation of the fission products of this Alga into Diatoms—that is, the Anabena and the Chlorochytrium may be found mixed in the most varied proportions, or the Chlorochytrium may be completely replaced by the Anabena. Moreover, in specimens of the former kind, the individual elements of the Chlorochytrium may often be observed actually undergoing the process of transformation into Anabena, and changing in colour from green to blue, purple, or even red. This process of transformation may be seen taking place in a sub-epidermal space which

[1] One part of glycerine to two parts of a 2 per cent. solution of formalin

is still densely packed with the fission products of Chlorochytrium —and such transformed elements may be seen presenting themselves either in the centre of the mass or at its periphery. The typical colours of the Anabena show themselves first in separate corpuscles, while these corpuscles subsequently multiply so as to produce the necklace-like chains.

In Fig. 31 (× 375) some illustrations will be found of the foregoing statements ; thus A shows a space in which some small, and several large, green Chlorochytrium segments were found in association with purple Anabena corpuscles ; in B a larger space is shown, in which there was a nearly equal and diffused admixture of the green Chlorochytrium together with purple and blue Anabena elements ; in C a space is represented, distended by Anabena chains, which showed under the microscope only three or four minute Chlorochytrium segments remaining ; while in D some of the blue Anabena is shown as it was growing free, after having burst through some of the cells at the broken edge of one of the leaflets of the Lemna.

Seeing that the spores of Anabena are non-motile they would have no means of getting into these closed cells, and, apart from this general consideration, there is the fact that the change of colour and heterogenetic transformation has been actually seen occurring here and there in the midst of tightly-packed Chlorochytrium cells.

(c) On the Origin of Anabena from the Chlorophyll Corpuscles of Lemna trisulca.

An isolated, decolourised leaflet of *Lemna trisulca* was found in which, along the centre and towards its proximal extremity, green chlorophyll corpuscles were still to be seen lining the spherical, sub-epidermal cells. In two separate, lateral regions of this leaflet ill-defined patches were found in which the chlorophyll corpuscles were mostly decolourised, and showed only a few colourless granules in their interior. Intermixed among them, however, there were other chlorophyll corpuscles which still preserved their green colour. A portion of one of these patches is shown under a low power in Fig. 32, A (× 250).

As I wished to see what further changes these corpuscles would undergo, I placed this leaflet alone in a small tube with some fresh water, and left it under a bell-glass on my mantel-piece. Three

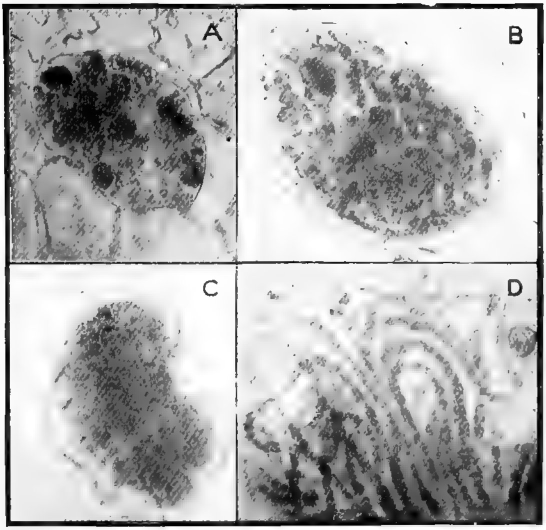

FIG. 31.

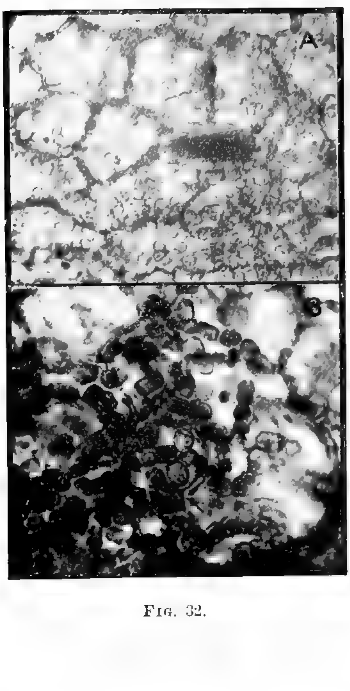

FIG. 32.

To face p. 210.

weeks later I examined the leaflet again, and found, in the same patch, that some of the corpuscles were still single and colourless, while others of them were pale blue-green, or pale blackish-purple in colour : intermediate conditions were also recognisable. Those that were coloured in the way I have indicated were either single, or had grown into chaplets of three, four, or more elements such as are shown in B (× 500). The same kind of changes were going on in the other patch of decolourised chlorophyll corpuscles. *Yet all of them were contained within closed spherical cells covered by an uninjured epidermis on each side of the leaf.*

For two weeks more I examined this leaflet from time to time, and found the same changes slowly progressing. Soon afterwards they seemed to cease. The continued examinations, as well as the confinement indoors in a small tube beneath a bell-jar, were doubtless not a little unfavourable. Still the trichomes of Anabena grew in length, and increased in number, varying somewhat in tint, but being mostly of a pale purplish-black colour.

Here again the heterogenetic transformation was actually seen occurring within the closed cells of the Duckweed ; into which the *non-motile* spores of Anabena had no means of getting.

(d) Transformation of the Contents of the Resting Spores of a small Spirogyra into a number of Amœbæ.

A specimen of a small Spirogyra (known as *S. quadrata*), having the lateral mode of copulation, was found in the month of April in a small pool from which most of the water had evaporated. A portion of this weed was placed in a shallow pot on the mantelpiece, only just covered by water, and under a small glass shade. After about one week the weed began to die, and towards the end of the next week many of the resting spores (which survive and remain green much longer than the filaments in which they have been formed) were seen to be undergoing the changes now to be described.

The resting spores of this species of Spirogyra have an ellipsoidal shape, and on examination with the microscope their green contents can be seen to be largely composed of the nuclear-like bodies of the bands of the two cells by whose fusion the spore has been formed. These bodies may be recognised in Fig. 33, A (× 250). In other specimens some of these bodies seem to have fused so as to give rise to a smaller number of rather larger

sub-spherical masses, such as are to be seen in B. Here they do not differ much from the bodies seen in A, though in C they are distinctly different—not only less numerous, but beginning to decolourise in both the specimens shown. In D the decolourisation has become more complete and the embryo Amœbæ (seven or eight in number) more distinct, and mixed only with a small amount of refuse matter in the form of reddish-brown pigment granules. In E the decolourisation of the Amœbæ is still more obvious, and two of them have made their way out, and are lying one of them above and another at the right extremity of the resting-spore. Among the many specimens I have examined one, rather larger than usual, was seen containing a dozen or even more of these Amœbæ. The wall of the spore was thinned and about to give way.

Here again the facts seem only capable of explanation on the hypothesis of Heterogenesis.

As in the case of the large Confervoid cells, the transformations of whose endochrome I have described elsewhere,[1] the masses which subsequently develop into Amœbæ seem evidently to be actual segments of the cell-contents. They are not seen at first of smaller size, and gradually enlarging, as might be expected if minute Amœbæ had entered from without, and had gradually devoured the contents of the resting spore. What is really seen is that the contents of the resting spore aggregate into 6–12 green masses, whose size remains fairly constant, but whose substance undergoes a progressive decolourisation and metamorphosis; with the result that just so many colourless Amœbæ appear, and make their way out from the thinned envelope, leaving behind them only a few refuse pigment granules.

(e) Transformation of the Substance of Vaucheria Resting Spores into Amœbæ.

The transformation of Vaucheria resting spores into Amœbæ is one which I have only seen on a few occasions, so that as yet I have not been able to trace all the stages of this change in a complete manner.

In Fig. 34, A (× 375), one of the thick-walled and very large resting spores of *V. Dilwynii* is represented. It was filled with

[1] "Studies in Heterogenesis," pp. 3–9.

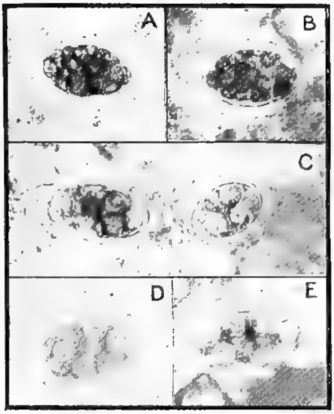

FIG. 33.

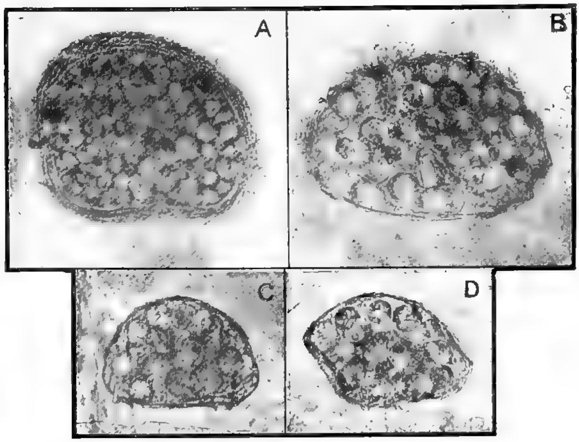

FIG. 34.

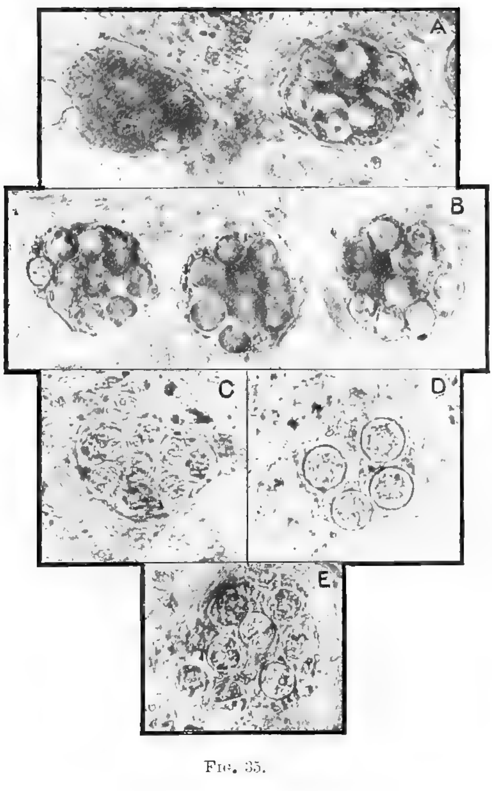

FIG. 35.

To face p. 212.

the usual light-green, fatty-looking corpuscles. In B another of these bodies is shown which was crowded with motionless, resting Amœbæ, the largest one, below, possessing a distinct nucleus. They were decolourised, but mixed up within the cyst with refuse granular pigment.

In C another smaller resting spore is seen full of much more rudimentary spherical masses, whose substance was distinctly and coarsely granular, rather than composed of clear protoplasm as in the nucleated specimen above referred to. These specimens were found in the midst of a mixture of Vaucheria and Spirogyra filaments, and the smaller one was kept under a cover-glass for three days in the hope that development of the organisms might proceed, distilled water being added day by day. It is rare, however, for these changes to proceed much under such unfavourable conditions. The photograph D was taken at the end of this period. The spore was somewhat pressed out of shape by the daily subtraction and addition of water, preceding and following examination with the microscope, but the Amœbæ had become more distinct, rather less granular, and two or three of them showed the appearance of a nucleus.

(f) Transformation of the Substance of Euglenæ into Amœbæ.

The production of Amœbæ from the substance of Euglenæ is a change that takes place with great frequency, both in small and in large specimens, when they are in an encysted condition. In the former case only one to three Amœbæ may be produced, but in large specimens as many as ten or twelve may be produced within a single Euglena. After their production the Amœbæ remain as motionless spheres, it may be for two or three weeks, gradually increasing in size, while the original Euglena cyst slowly disintegrates or softens, and disappears.

If a portion of a Euglena pellicle is kept for a week or ten days in the dark, this kind of transformation is very commonly to be seen in some of them.

The photographs which I have selected for illustrating this process were taken from some large Euglenæ obtained about the middle of September, 1900, from the border of a small lake at Loughton, where they formed a pale but bright green coherent film. After they had been kept indoors in a dim light for ten days

such changes as I am about to describe were occurring simultaneously in hundreds of them.

The first indication of this change is represented in Fig. 35, A (× 375), where in the Euglena to the left, with contents granular but still green, there is an indication of some large spherical bodies forming within ; while in the specimen on the right the bodies are more distinct and partly decolourised. In the three specimens shown in B, the whole substance of the Euglenæ is seen to have become converted into such bodies, except for a small amount of refuse granular matter, of a red-brown colour. In the specimen to the right the decolourisation of some of the spheres is more complete —their substance being homogeneous, and as yet unlike that of an Amœba. Fig. 13, C, represents one of these Euglenæ that was photographed ten days later, and it will be seen that while the cyst wall has become more indistinct the Amœbæ have become much more definite, the granules between them having for the most part disappeared, while their own substance has become much more granular—probably in part owing to their having swallowed the pigment granules previously in contact with them. After another ten days, in the last remnants of the original Euglena pellicle, I found the specimen represented at D, in which the Amœbæ had distinctly increased in size, while only very faint indications existed of the outline of the Euglena in which they had been formed. I have seen some specimens like this slowly changing in form, and beginning to take on an active existence. In E the lowest of the Amœbæ will be seen to have undergone fission ; and this is the only instance in which I have ever seen evidence of fission in these motionless Amœbæ while still within the Euglenæ. The one to the right of that in which fission had occurred showed a distinct nucleus.

Great numbers of the Euglenæ were seen to be undergoing the same kind of change ; dividing into separate, motionless bodies, remaining of the same size, gradually becoming decolourised, and undergoing molecular changes which converted them into motionless amœboid spheres—while no active Amœbæ were to be found outside, and there was an entire absence of any evidence of infection.

(g) Transformation of Chlorophyll Corpuscles of Nitella into Amœbæ.

I have long been familiar with a difference in the rapidity of

growth of plants of Vallisneria in West Middlesex water and in the New River water respectively. They have always seemed to grow much more rapidly and vigorously in the latter than in the former, a fact doubtless due to some differences in the saline constituents of the waters, and the greater abundance of lime salts in that from the New River. When tap water has been mentioned hitherto it has always been West Middlesex water that has been referred to. I wished, however, to ascertain what would be the result of keeping Nitella for a time in New River water, and accordingly, towards the end of 1898, I made the following experiments and observations.

Fresh specimens of *N. flexilis* were kept for four or five weeks in an open vase on the mantelpiece containing this New River water, and portions were cut off, from time to time, and placed in small covered, earthenware pots containing distilled water only, and allowed to remain there for a variable number of days.

On several occasions, after only three or four days of this change of conditions, I have found many of the previously healthy cells completely altered, and that often in two different ways in contiguous cells. In each case, however, the cells had completely lost their bright green tint and had assumed either a spotted appearance or a more uniform drab or earthy colour, as shown in Fig. 36, B (× 25).

Two of the cells there represented, which showed the uniform drab or earthy colour, were found to be crammed from end to end with a continuous layer of minute motionless Amœbæ, which had completely replaced the layer of chlorophyll corpuscles. This extraordinary result is shown in Fig. 37, B (× 375), as it occurred in a terminal axis cell, which was in a healthy condition when placed in the pot three days previously. Many of these minute Amœbæ contained brownish or olive-coloured residual granules. Between them a few motionless Bacteria were to be seen here and there ; but not a single green chlorophyll corpuscle was left.

This particular change has not been confined to specimens of *N. flexilis* treated in the manner above stated. I had not unfrequently met with it before I began to make use of the New River water, as my notes show, and always in association with the common change — that is, the two changes often occurred in contiguous cells in the most puzzling manner. I can find no details as to the conditions under which this particular change was

serve as food for the young Amœbæ, which consequently attain altogether larger dimensions. The inequality in size is very marked in B.

In all the cases where this kind of change is taking place I have observed again, as on previous occasions, that there is a more or less thick layer of motionless Bacteria outside and between the Amœbæ, and, of course, on the inner side of the wall of the cell, while the surface of the wall is thickly flecked with a number of curious glistening white specks, such as may be seen in Fig. 38, C. A little above the proper focus these specks look black instead of white. Another interesting point is shown by this specimen. It was taken from one of the larger sub-terminal cells, in which the chlorophyll corpuscles were larger than they are in the minute terminal cells, and many of them also contained rather large starch grains, such as are common within the chlorophyll corpuscles. Several of such grains may be seen, still undigested, within the Amœbæ into which the chlorophyll corpuscles have been converted. It cannot be supposed that the starch grains are now in the Amœbæ, because the latter have swallowed the entire chlorophyll corpuscles. There was not the least evidence of this, and, moreover, the small size of the Amœbæ forbids such a notion.

(h) Transformation of the Substance of Euglenæ into Peranemata.[1]

The change of Euglenæ into Peranemata has been followed on several occasions, but I have never seen such multitudinous examples of this transformation as occurred in a pellicle composed of large Euglenæ, brought from the small lake near Loughton—when thousands were seen undergoing this change ; just as others, equally numerous, were becoming converted into Amœbæ in the manner shown in Fig. 35. These two changes occurred during the same period. They were first recognised about ten days after the pellicle had been obtained, and when the Euglenæ were all encysted. The two transformations sometimes occurred side by side, but the change into Amœbæ took place most abundantly in a portion of the scum which was floating on water in a deep bowl, while the change into Peranemata was most common in portions of the scum that had been placed in a shallow saucer.

The beginning of the change into Peranemata is shown in

[1] These are organisms something like colourless Euglenæ, though provided with a much stouter flagellum.

Fig. 39, A (× 250). The Euglenæ become granular and partially decolourised, and careful examination shows, at first, a dim indication of spherical bodies such as are to be seen in the left hand specimen. These become rather more defined as decolourisation progresses, as shown in B (× 375). These particular embryo Peranemata are distinguished from the embryo Amœbæ, from the first, by the number of refractive particles seen in their interior, which seem to enlarge as development proceeds. Such biscuit-like particles are particularly distinct in the right hand specimen of B, but are also obvious in C—both in the encysted Peranemata and in the two free specimens. These latter were active, owing to movements of their long and stout flagella, before they were rendered motionless by a weak formalin solution, which unfortunately had the usual effect of causing the flagella to be retracted or curled under the body of the organisms.

It will be observed that here, as in the case of the Amœbæ derived from the substance of Euglenæ, the spheres, which become organised into Peranemata, on their first appearance are motionless and already fill the Euglena cyst; and that in the process of organisation they neither increase appreciably in bulk nor in number. These facts are thoroughly in accordance with the view that they are products of heterogenesis; and are equally adverse to the hypothesis that the Peranemata, as found, are results of infection. No theory of infection could possibly account for the first appearance of the organisms as motionless, densely packed, green spheres gradually becoming decolourised; and for the fact that this same change occurred in multitudes of Euglenæ, when at first not a single free Peranema was to be seen.

These organisms are, however, sometimes produced from small encysted Euglenæ in a very different manner, and they then commonly have quite a different appearance, owing, for one thing, to the absence of the large biscuit-like particles which were so distinctive in those last referred to. The encysted Euglena becomes decolourised, except for some few green, brown, or yellow pigmentary aggregates—or perhaps wholly decolourised. Then the entire mass after a time becomes converted, without segmentation, into a single Peranema, which gradually begins to revolve within its cell. At other times, a single segmentation occurs, and the products become converted into a pair of Peranemata. In Fig. 39, D (× 250), one of these small encysted Amœbæ is seen on the left; next to it, another is becoming decolourised, and in the specimen

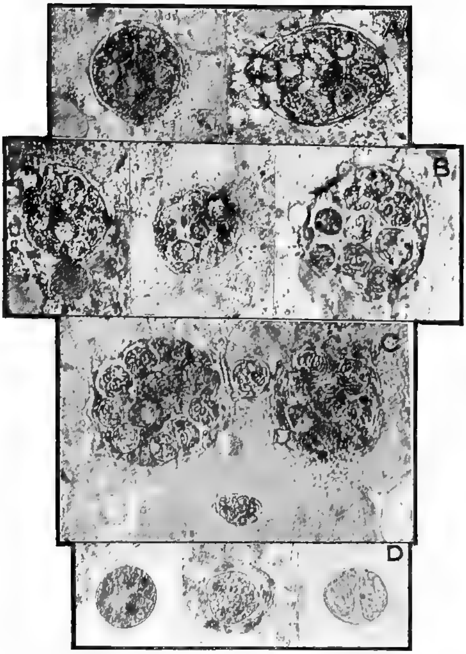

FIG. 39.

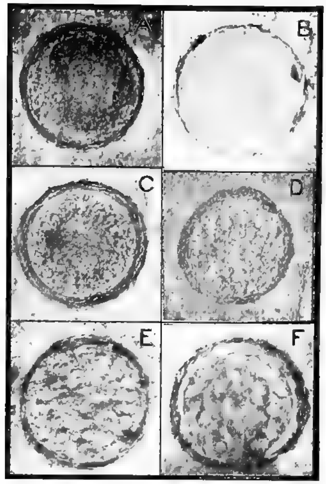

FIG. 40.

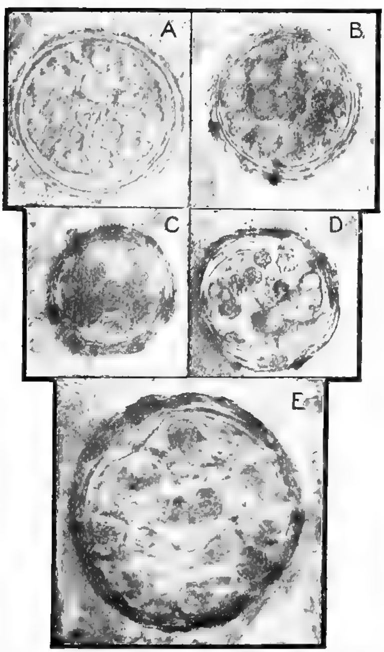

FIG. 41.

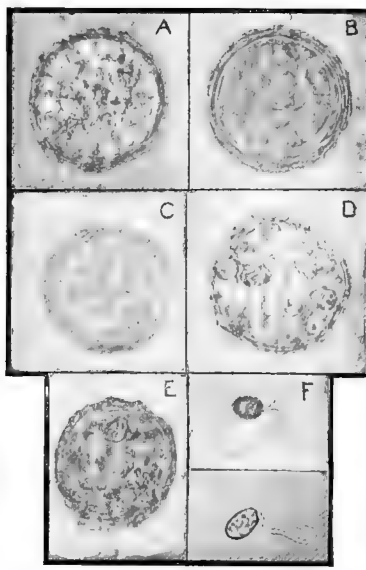

FIG. 42.

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on the right the contents of the cyst have been converted, without remainder, into two Peranemata, except that in one of them the pigment of the eye-speck was in part left. These bodies I watched for some time actively moving within their cyst, and the long stout flagellum of each was distinctly seen before their movements were stopped by a weak solution of formalin. I have also watched many others and have seen them emerge from their cysts as rather large, fully-formed Peranemata. Many of them contain small, differently-coloured pigment masses, which are small unconverted, or partially converted, portions of the substance of the Euglenæ from which they have been formed. These organisms never seem, like Amœbæ, to take masses of food-stuff into their bodies.

(i) Transformation of the Substance of Encysted Prorodons into Peranemata.

The changes now to be described occurred in a number of encysted Ciliates that were found in a tall vessel, filled with water to the depth of about seven inches, into which, six weeks previously, and near the end of the month of October, an abundance of Chlamydomonads and Euglenæ were placed. The vessel remained during most of this time on a stand outside a window with a south aspect. After about four weeks a scum had grown over the walls of the vessel, which was found to consist to a large extent of a felt-work of algoid filaments, mixed with Oscillatoriæ, and a number of Euglenæ. The Euglenæ were most abundant in the scum over the lower third of the vessel, while in the upper third, more especially, there were found a large number of active Prorodons (with distinct terminal oral cylinders) mostly gorged with small green corpuscles: other specimens were seen becoming motionless and beginning to encyst; while a much larger number of them were actually encysted and decolourised—the cysts being very thick, plicated, and without spines or projections of any kind.

These encysted specimens were searched for and examined at intervals for a period of about three weeks, by which time the supply was pretty well exhausted. The following are some of the facts that were made out.

The tendency in the great majority of them was to reach a certain stage of reorganisation, and not to advance any further; the conditions apparently not being favourable to the occurrence of the ordinary changes which these encysted Prorodons were

accustomed to go through. And perhaps this might have been, in part, due to the fact that, three or four days before the date of my first examination of the scum, I had put a glass cover over the jar, and had consequently cut off the supply of fresh air. This undoubtedly had a very injurious effect upon some of the organisms on the sides of the vessel, as over the lower third there were a number of small red worms in loose sheaths diligently devouring the scum before the cover was applied: but when I began my examination I found some of these worms dead within their sheaths, as well as very many of the latter empty, and not a single one of them containing a living worm.

Several different kinds of changes were seen taking place within these encysted Prorodons which I have elsewhere described ("Studies," pp. 20–24); but the particular transformations now to be referred to were met with most frequently in portions of the scum which, after having been scraped off the sides of the glass, were subsequently kept for two or three days, previous to examination, in such compressed or rolled-up masses in a small vessel containing distilled water. Other specimens, however, have also been found undergoing similar changes in portions of scum examined directly after they had been removed from the walls of the tall glass vessel. The commonest of the changes found was one by which the entire contents of the cyst ultimately broke up into a number of very coarsely granular spheres, each of which developed into a Peranema.

During the early stages of this change the encysted matter, as a whole, loses its ordinary granular appearance; it becomes more refractive, and with what would be an approach to translucency were it not closely studded with coarse, fatty-looking particles (Fig. 40 (× 250) C). Soon, traces of separation into distinct units begin to show themselves as in D and E (× 250) and Fig. 41, A (× 500). Later still, spherical bodies of variable size actually separate from the parent mass; so that the whole cyst may be seen to be completely filled with a closely packed aggregate of motionless and coarsely granular spheres, as shown in Fig. 40, F.

At other times the units that separate from the matrix may be of fairly equal size and larger, but leaving a certain amount of unconverted residual matter, as in Fig. 41, B (× 250). Here the units not being so closely packed showed slow, semi-rotatory or pendulum-like movements, and had to be killed with a weak formalin solution before the photograph could be taken. In another cyst there were

found six coarsely granular units varying much in size, some of which were exhibiting the same slow, semi-rotatory movements, though no flagella could be detected (Fig. 41, C, × 250). Some refuse granular matter was seen among them, and it seemed probable that some of the Peranemata had escaped, as the cyst was not nearly full. A very similar condition of things existed in another more highly magnified specimen, only in this case the organisms exhibited rather freer movements within the half-empty cyst (Fig. 41, E, × 500); and languid movements of their long flagella were distinctly seen. After the application of the formalin solution these flagella, as is generally the case, were no longer visible, and are consequently not to be detected in the photograph.

In these two photographs all the stages are shown by which the encysted mass becomes resolved into a closely-packed aggregate of *motionless* spheres within the thick-walled cyst. Had they been products of infection they should have been preceded by an active stage—which was absent. The active stage came later, with further development of the spheres into Peranemata.

(j) Transformation of the Substance of Encysted Stylonychiæ into Monads and Peranemata.

Changes very similar in nature, though not quite so varied, have also been traced in another set of matrices which, for about three months, remained without showing any appreciable change. It was only after that period that some of them began to undergo developmental processes, resulting in the production of Ciliates of the kind known as *Stylonychia lanceolata*. The fully developed matrices were contained in fairly thick cysts, covered with short conical projections, almost exactly like what are figured by Saville Kent in his "Manual of the Infusoria" as pertaining to one of the other representatives of the genus Stylonychia.

As I have said, these matrices remained for about three months without undergoing any further change. Then, within a few of them, Stylonychiæ began to develop, to rotate, and ultimately to emerge from their prison. About the same time that these normal developments of the contents of the cysts were first observed I began also to find other cysts, in which the contents were undergoing, or had undergone, certain heterogenetic transformations.

About ten weeks previously, portions of an Euglena pellicle in which they were found had been transferred by a section-lifter

recognised life-history of Peranemata or Monads will, in any way, explain such facts, or the appearances represented in the photographs.

(k) On the Origin and Development of Swarms of Actinophrys from the Chlorophyll Corpuscles within Nitella Cells.

In the cells of Nitella, dying soon after they have been brought from the ponds, specimens of Actinophrys may often be met with, more or less sparsely distributed, in association with small Amœbæ, within the cells of *N. flexilis* and *N. translucens.* The two organisms are commonly of just the same size ; they contain from 4–8 chlorophyll corpuscles, one or two only of which may be seen to be reddish-brown in colour while the rest have remained green. Such bodies, with or without rays (Actinophrys or Amœbæ), are commonly met with either in association with, or anterior to, the presence of certain small Ciliates, which are also frequently to be found in large numbers within Nitella cells.

I am not now about to say anything further concerning this mode of appearance of Actinophrys, but rather as to their appearance in an altogether different fashion, and in prodigious numbers, *pari passu* with the disappearance of the chlorophyll corpuscles of the cell.

This change has occurred under different conditions, but I will first refer to a set of observations which I have repeated on four occasions with very similar results.

About thirty bright green, terminal sprigs from a stock of *N. opaca*, which had been in the house for some weeks, were placed in a covered, glazed metal basin containing equal parts of tap and distilled water. During the remainder of the experiment, the basin and its contents were kept at a temperature of about 60°F. At the end of a week, on removing the cover for a mere momentary inspection, very many of the cells were seen to have become decolourised, and to be of a pale earthy tint. Five days later, almost all the cells had become decolourised in the same way —only one here and there still remaining green.

Microscopical examination of several of the cells showed them to contain multitudes of Actinophrys, many being absolutely filled with them. In most of the youngest terminal cells (those probably which possessed the highest vitality) there was the production of the ordinary Amœboid spheres, such as are shown under a low

power in Fig. 36, A, going through their usual changes[1]; though one or more cells in many of the groups were found to present none of these organisms, and to be crowded with specimens of Actinophrys only. All the sub-terminal, and other larger cells, were found to be in a similar condition and crammed with Actinophrys; the only other organisms met with in these cells being dense masses of motionless Bacteria—an association which I find to be invariable in this kind of change. It is one which does not take place simultaneously throughout the whole length of the cells, but extends steadily from one end of the cell to the other. Roughly speaking the order is this: the Bacteria multiply more or less notably, while the green chlorophyll corpuscles disappear, and are replaced by specimens of Actinophrys.

Where this change is advancing through a cell, careful examination will always show a mixed zone intervening between the healthy and the changed regions of the cell—that is, between its green and its drab areas. In the latter region nothing but a dense mass of Actinophrys, mixed with Bacteria and starch granules, is to be seen—some of the specimens of Actinophrys being completely decolourised, while others show a few green, mixed with colourless, granules in their interior. Next to this area of complete change we have an intermediate zone, in which *small* specimens of Actinophrys and chlorophyll corpuscles are intimately intermixed; while beyond it we have green chlorophyll corpuscles only—all of them more or less loaded with starch granules or scales. This particular change being most prone to occur in old specimens of Nitella which have been kept for some time in the house and then treated as I have described, it will commonly be found that the chlorophyll corpuscles are, in such specimens, crowded with starch granules and are, as a consequence, *very greatly enlarged.*

A more thorough and searching examination of the intermediate zone will show that all the smallest specimens of Actinophrys are of just the same size as the chlorophyll corpuscles—that *none are to be found smaller than these corpuscles*; that they have, at first, no rays; that they are motionless; and that not one of them can ever be seen to divide.

What then is the mode of origin of all these myriads of Actinophrys which, within a few days, will appear within the closed cells of the Nitella? Do they come from without? This

[1] These changes have been very fully described in my "Studies in Heterogenesis," pp. 245–266.

view is negatived by the fact that none are ever to be seen making their way through the thick walls of the Nitella cells; though, looking to their large minimum size, to their prodigious numbers, and to the fact that they are never seen to divide, thousands of them ought to be capable of being seen boring their way through the walls of the cells, had they come from without. Further, these embryo organisms are always *completely motionless, and yet first show themselves intimately intermixed among the chlorophyll corpuscles.*

It comes therefore to this: myriads of chlorophyll corpuscles at first exist, and then become replaced by myriads of motionless specimens of Actinophrys, first showing themselves as bodies of about the same size as the chlorophyll corpuscles—a trifle larger but never smaller. How do the chlorophyll corpuscles vanish? And whence come the specimens of Actinophrys? These are the two related problems; and the only answer is, that the myriads of chlorophyll corpuscles are converted into the myriads of Actinophrys—just as, under other conditions, and in young Nitella cells whose chlorophyll corpuscles contain no starch grains, they may be simultaneously converted into minute Amœbæ, such as I have already described, and represented in Fig. 37.

The mode in which the transformation of the chlorophyll corpuscles into the Actinophrys is brought about seems to be this. The corpuscle appears slightly to enlarge, and, at the same time, begins to decolourise, leading to the production of colourless mixed with fine green granules. The starch grains also begin to be digested. We have here the first stage of the motionless Actinophrys; spherical and, as yet, showing no rays. No entire chlorophyll corpuscles are ever to be seen within these specimens of Actinophrys—only, at first, a few green granules.[1] These granules soon disappear as the organisms slightly increase in size; and where they are not too densely packed they may be seen to emit 6–8 pseudopodia, whose length about equals the diameter of the Actinophrys. Some of the chlorophyll corpuscles, failing to undergo this transformation, become disintegrated and liberate their starch grains, and such scanty food is devoured by those of the new organisms which come into contact therewith. The paucity of food, however, in comparison with the myriads of

[1] It cannot be, therefore, that the chlorophyll corpuscles are swallowed by the Actinophrys. The latter is of the same size as the chlorophyll corpuscle when, in its rudimentary form, it first shows itself among them.

organisms, accounts for the fact that the organisms grow very little, and display only comparatively slight differences in size—as the figures will show.

I have seen this kind of change occur in recently cut cells as well as in entire cells. And when it occurs in cut cells, it sometimes gradually spreads from the open extremity inwards ; but just as frequently I have seen it begin at the inner and closed extremity of a cut cell and gradually spread outwards.

Some illustrations will now help the reader more fully to realise what I have been describing ; but, as photographs can only show the crude results rather than the minute details of this process, it seemed best to reserve them till the description of what occurs has been given.

In Fig. 43 (× 150) a portion of a cell of *N. opaca* crammed with recently formed Actinophrys, together with partially decolourised chlorophyll corpuscles and masses of motionless Bacteria, is shown under a low power. Many of the specimens of Actinophrys still contained green granules in their interior, though others were completely decolourised.

In *N. opaca* the chlorophyll corpuscles are always much larger than in either of the other two species, and when they are filled with large starch scales they undergo a further notable enlargement, as was the case in the cell from which Fig. 44, A (× 375) was taken. This figure shows an intimate mixture of enlarged chlorophyll corpuscles packed with starch scales, and embryo specimens of Actinophrys, partly decolourised, into which other of the corpuscles have become converted. All stages between the two were to be seen in this specimen. They were all motionless and the photograph was taken without the use of any lethal reagent. All that was to be seen on the surface was an accumulation of Bacteria and Leptothrix.[1] In B (× 200) a portion of a completely decolourised cell of *N. translucens* is shown crammed with colourless specimens of Actinophrys. Being more developed, these organisms are liable to exhibit slight though scarcely perceptible movements, so that a weak solution of formalin was used before the photograph was taken. Bearing in mind the small size of the chlorophyll corpuscles in *N. translucens* it will be noted that, where they are small, the specimens of Actinophrys are similarly small.

[1] The surface of the cells represented in Figs. 43, 44, B, and 45, A were even almost free from such organisms ; and showed not the least indication of Amœbæ or Actinophrys penetrating from without.

Comparison of Fig. 44, A and B will make this plain, even when due allowance has been made for their different degrees of enlargement. The same greater size of the specimens of Actinophrys, with which another cell of *N. opaca* was densely packed, is shown in Fig. 45, A (× 125). The organisms here were all completely decolourised, and, through the centre of the cell, a large dense aggregation of Bacteria was to be seen, in the midst of which many of the Actinophrys were embedded.

I have tried on several occasions to get photographs of these specimens of Actinophrys, but with no success. All reagents have proved useless because they, at once, entail a retraction of the pseudopodia. And, however motionless the Actinophrys appears to be, the photograph generally shows that it is only relative. The best I have been able to do is shown in my "Studies in Heterogenesis" in Fig. 177, A (× 375), which was taken with a very rapid plate and an exposure of one minute. The nucleus and nucleolus are to be dimly seen, together with six or more rays; and this represents the common appearance of the organism in its active state. After from about seven to ten days, when their scanty food is more or less exhausted, this active state is over, and the organisms then encyst themselves, when they present the appearance shown in Fig. 45, B (× 375).

Once in the encysted condition, specimens of Actinophrys seem to remain long without undergoing any further change. I kept some of the above described specimens in a small tube for about six weeks, but at the expiration of that time they showed no appreciable alteration, except that the enclosed protoplasm seemed a little less granular than it had been previously.

I have already alluded to the fact that the Monad, the Amœba, and the Actinophrys are only different phases easily convertible the one into the other. This is thoroughly recognised as regards the Monad and the Amœba, and fresh evidence thereof is constantly to be met with in studying the results of segmentation of the common Amœboid spheres in Nitella—sometimes the products appear as Monads and sometimes as Amœbæ, without any reason for this variation being apparent; and a similar interchangeability between these two forms was met with in the colourless segmentation products of Zooglœa, as already described in Chapter X. During my examination of the specimens of Actinophrys crowding the cells in *N. opaca*, wishing to preserve one of the cells mounted

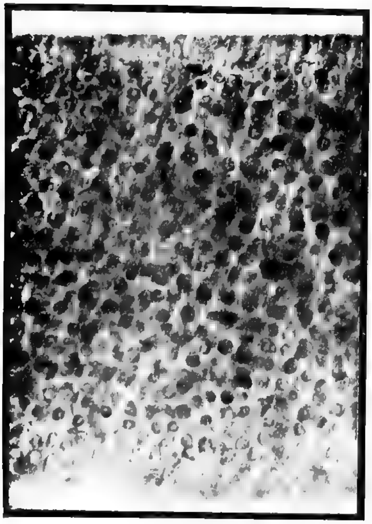

FIG. 43.

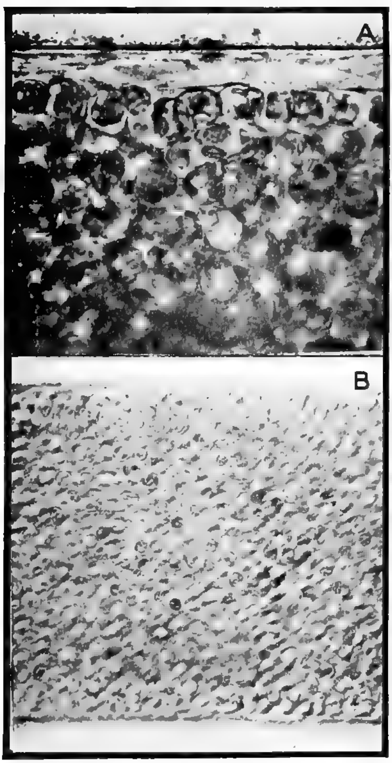

FIG. 44.

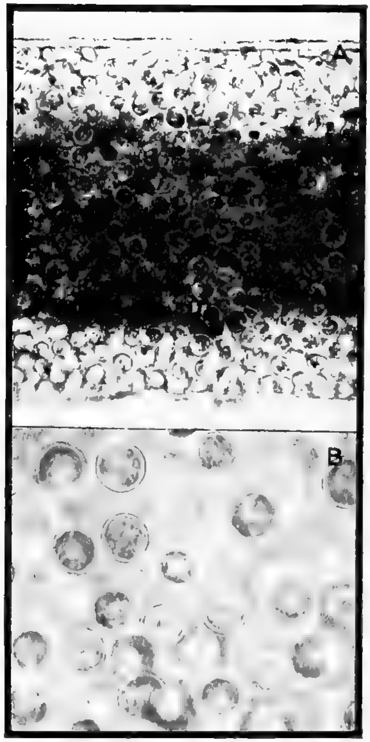

FIG. 45.

To face p. 228.

in water for future observation, I smeared round the edge of the cover glass with vaseline, so as to prevent evaporation, and on examination, after an interval of twenty-four hours, all the specimens of Actinophrys were found to have been converted, in this confined situation, into typical though rather sluggish Amœbæ.

If, moreover, we bear in mind that, in most cases, the small masses of Zooglœa which ultimately segment into Monads or Amœbæ are absolutely indistinguishable from others which finally segment into brown Fungus-germs; and that, from an encysted Euglena or an encysted Ciliate, Amœbæ, Monads, or Peranemata may take their origin, it looks as if we have in these lower forms of life—the Monad, the Amœba, the Actinophrys, the Peranema and the Fungus-germ—forms which are to a certain extent, and under varying conditions, interchangeable with one another.

This will, of course, be absolutely at variance with Weismann's doctrine of 'determinants'; but would not be incapable of receiving an approximate explanation in accordance with my view that living matter is still constantly coming into being endowed with a wonderful plasticity; and that the early forms which it assumes are determined by the 'polarity' of its molecules, in just the same way that the forms of crystals are a necessary outcome of the polarity of their molecules.[1]

We have seen (pp. 44–46, 63) how surprising are the variations in form and colour of this or that crystalline substance, under the influence of slight changes in the conditions amidst which it is formed. So that new-born living matter, with its extreme molecular complexity favouring isomeric changes, ought also to be capable of displaying kaleidoscopic variations in form. From this point of view, the extreme variability of Bacteria, their fusion into higher forms of life in Zooglœa aggregates, and all the evidence as to transformations and interchangeability in the form and nature of lower organisms adduced in this chapter may be deemed to harmonise with the opinions expressed in this work, though such facts are irreconcilable with commonly received doctrines concerning the origin, nature and age of these various lower forms of life. In accordance with generally received views coupled with the doctrine of Evolution, they ought, all of them, to have totally disappeared from the face of the earth millions of years ago.

[1] We are driven to the conclusion, in fact, that the simpler the organisms with which we have to deal, the weaker will be the influence of Heredity; and that, in new-formed living matter, this conservative tendency of Heredity would be no more potent than it is in crystalline matter.

CHAPTER XII

THE HETEROGENETIC ORIGIN OF CILIATED INFUSORIA

(a) On the Appearance of Ciliates in Infusions prepared from Hay and other Plants.

IF Ciliates are to appear in infusions of hay or other plants, the infusions should be made by steeping the plants for two to four hours in water at a temperature of 70–80° F. The fluid should then be filtered into a clean beaker, or other glass vessel, to a depth of three or four inches, through two layers of fine Swedish filtering paper. The vessel containing the filtered fluid may be placed under a bell-jar, or have its top loosely covered with a cap of filtering paper in order to protect the surface from the advent of much dust. The infusion may be either left exposed to ordinary daylight or placed in a dark cupboard without apparently influencing the result very much, so long as the temperature in the two situations remains about the same. The temperatures proving most favourable for varied changes in the infusions seem to be those lying between 60° and 75° F. Temperatures higher than this are, as I have found, less likely to lead to the production of Ciliates in the pellicles that form on most infusions.

The fact, as I have shown elsewhere,[1] that Ciliates have not appeared when the infusions have been made from immature and living grasses, and that one kind of Ciliate, and one only, speedily appeared when the infusions were made from ripe and recently dead grasses, must be regarded as facts of cardinal importance. It speaks against the notion of the appearance of the Ciliates in the filtered infusion being due to the fact that the grasses are contaminated with such organisms either from the air or by others which may have made their way up from the soil. If they had come from either of these sources they should have shown themselves in

[1] "Studies in Heterogenesis," p. 87.

infusions made from living grasses just as certainly as in those made from dead grasses.

The Ciliated Infusoria which, so far as my observations go, have been met with in hay infusions, and, with few exceptions, in the other vegetal infusions with which I have experimented, have all been species of Kolpoda. The specimens vary, however, much in size, not only in different infusions, but also, to a lesser extent, in the same kind of infusion. Some of the largest matrices from which these Ciliates issue are to be met with in hay infusions, where I have often seen them as much as 1/350 inch in diameter. One of these bodies is shown in Fig. 48, A (× 375). On the other hand, about the smallest Kolpodæ I have ever found have been encountered in an infusion prepared from Yellow Bed Straw (*Galium verum*), where many of them have not been more than 1/2000 inch in diameter. They have been small also in infusions made from Dutch Clover (*Trifolium repens*), though not nearly so small as in the previously-named infusion. Notwithstanding this great variation in size, the Kolpodæ are in other respects very similar; being fairly rotund, somewhat reniform in shape, and covered with short cilia, as in Fig. 48, C (× 375). Each organism has a single, large, spherical nucleus (not easily visible without the aid of stains), and a much more obvious large, contractile vesicle at its posterior extremity.

These Ciliates make their first recognisable appearance in the substance of the pellicle, in considerable numbers, usually somewhere between the fourth and the eighth day. They appear as spherical encysted masses, having the appearance in their primary stage of being mere individualised portions of the substance of the pellicle itself, bounded by a very delicate limiting membrane. Such bodies I shall continue to speak of as Ciliate 'matrices.' The contents of each gradually becomes converted into an embryo, which may be seen rotating within the limiting membrane—by this time slightly thicker and constituting a cyst, though still very thin. The embryo may perhaps undergo fission once, twice, or rarely even thrice before the individual organism, or the products of fission, are enabled to free themselves from the enclosing cyst, and appear as active, free-swimming organisms (Fig. 48, B, × 375).

What may be made out as to their formation and ultimate composition is illustrated in part by Fig. 46, in which the different specimens represented have been obtained, somewhere between the third and the fifth days, from the pellicles on different infusions. In

A (× 500), we have an aggregate of Bacteria, as an individualised portion of a hay pellicle, slightly stained with chrysoidin, and separating from it in the same kind of way that we have seen the discrete corpuscles and Zooglœa masses individualising themselves and separating from the pellicles in which they have been formed. In B (× 500), we have an unstained specimen which is obviously a mere spherical aggregate of Bacteria similar to those existing in the pellicle around, and as yet without a definite bounding membrane. A similar identity between the Bacterial contents of the matrices, and those existing around them, will also be seen in each of the other specimens represented in this and in Fig. 47. So that in this important respect the embryo matrices agree exactly with what we found to hold good for the small Zooglœa masses. A matrix, stained with a very dilute eosinophile solution, which was found in an infusion of *Melica nutans*, is shown in Fig. 46, C (× 375), while in D (× 375), we have two spherical aggregations of Bacteria, the smaller of which is very distinctly developing a limiting envelope. In E (× 250), we have a small matrix found in a hay pellicle with many others, and staining exactly like them with mastzellen fluid. It is a mere aggregate of minute Bacteria though bounded by a distinct limiting membrane. In F (× 375), we have a larger matrix, unstained, and from a different infusion. Here the bounding membrane is very distinct, though there is, as yet, no sign of a nucleus within the enclosed mass.

This question, as to the early composition and development of the matrices, being so very important, other illustrations are furnished in Fig. 47. In A (× 375), a nucleated matrix stained with mastzellen fluid is represented from a hay infusion. It is again a mere aggregate of Bacteria having a distinct limiting membrane, though here there is also a distinct nucleus. Two of these rudimentary matrices, with clearly-defined limiting membranes, but no trace of a nucleus, from an infusion of Melica, are shown in B (× 375). The dark masses in this, and in the next two figures, are groups of brown Fungus-germs out of focus. C (× 250) shows a group of matrices in different stages of development from a hay infusion on the fifth day. The specimen from which this photograph was taken had been beneath a cover-glass for twenty-four hours previously, in a very dilute solution of mastzellen stain. The pale vesicular bodies are mature matrices which had, in the meantime, become much altered by the formation of numerous vacuoles. Four other immature matrices, however, are well seen. The one above and to the

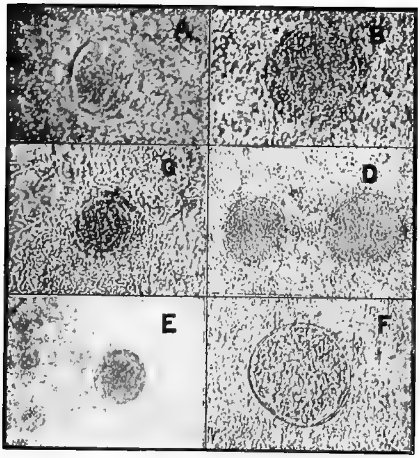

FIG. 46.

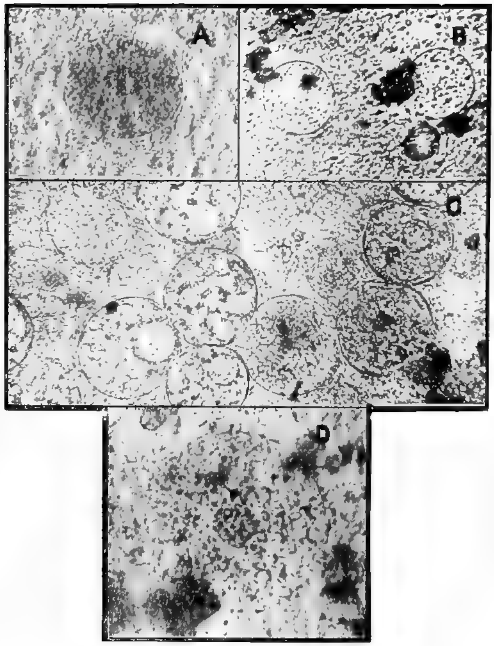

FIG. 47.

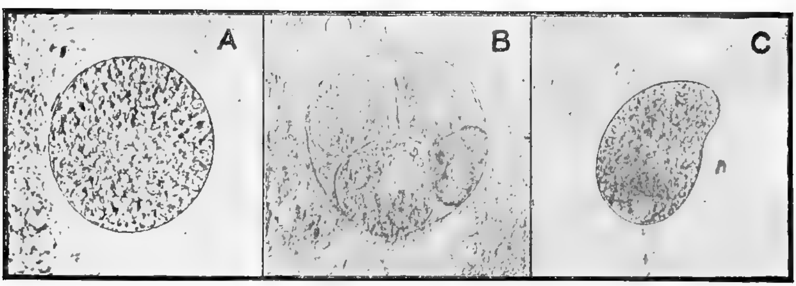

FIG. 48.

To face p. 232

left represents one of the earliest stages of individualisation from the pellicle ; while three, rather more advanced specimens, are situated close together to the right. The dark bodies obscuring them are, as I have said, brown Fungus-germs out of focus, but in each of these specimens nuclei in process of formation are to be seen. Another of these bodies, more enlarged, from the same specimen, is shown in D ($\times$ 500), in which development has advanced rather further—the nucleus being now very distinct, and having taken up more of the very dilute stain than the body of the matrix, and this again rather more than the surrounding pellicle. The similarity in composition between the substance of the matrix and that of the pellicle around it is well seen here, as in the other specimens: and actual examination under the microscope showed that the matrix had the same composition all through its substance.

The only Ciliates I have ever seen in the numerous hay or grass infusions, made in the manner already described, have been Kolpodæ, either large or small, in association with matrices, similarly varying in size. *These matrices*, as I have said, *appear in filtered infusions before the free-swimming Ciliates.* They appear in large numbers, in the course of a few days, in the substance of the pellicle. But in a few cases in which, in some early experiments, I added a small quantity of unboiled tap-water after the infusion had been filtered, another kind of Ciliate appeared in the infusions and rapidly multiplied therein.[1] In each case the Ciliates that appeared under these circumstances were of the same kind. They were Chilodons, though I have never seen the least evidence pointing to the origin of these Chilodons in the hay pellicles. No matrices of such Ciliates have ever been found in the pellicles when they have been present; nor have any of the Chilodons been seen in these infusions in a state of encystment. I have not the least doubt that the infusions were inoculated with the Chilodons when they were diluted with the unboiled tap-water.

It is the more notable, therefore, that the other hay infusions with which I have worked, though prepared with the same kind of tap-water and at temperatures under 100° F., should never have shown one of these Chilodons. It seems difficult to explain such a fact except on the supposition that the process of filtration adopted

[1] If water is added for any purpose to the infusion after it has been filtered, the addition ought always to be of water that has been recently boiled, or else of water filtered as the infusion had been.

had been adequate to exclude all such Ciliates and any possible germs to which they may have given rise. But if the filter could thus exclude contamination by Chilodons, it ought to be similarly potent in excluding Kolpodæ or any bodies (nature unknown) which may act as their germs.

The individualised portions of the pellicle, of the actual size of the Ciliate matrices ultimately produced (after their enclosure within limiting membranes), represent the first appearance of these bodies in properly filtered vegetal infusions. These primary matrices show themselves, as I have said, mostly in from three to eight days, and subsequently remain stationary in size. This may be termed *the direct origin of Ciliates from the pellicle*, in contrast to another mode, occurring later, now to be referred to, which can perhaps be best spoken of as *the amœboid origin of Ciliates in the pellicle.*

In this second mode of origin, the matrices, from minute beginnings, progressively increase in size; and when growth ceases generally become enclosed in thick cysts, often having a brown colour. These are rapidly formed and contrast notably with the often scarcely visible, pellucid cysts of the primary matrices.

This amœboid origin of the Ciliate matrices reveals itself also at comparatively late periods—mostly from the tenth on to the twenty-fifth day. They appear partly in the original pellicle, and partly in the numerous zooglœal, villous-like extensions therefrom into the fluid below, such as are commonly found on the under surface of old pellicles. Some of these extensions crowded with Ciliate matrices are shown in Fig. 51, A (× 15).

They begin as minute corpuscles, which I have traced down to about 1/5000 inch in diameter, and gradually increase in size up to that of small or medium-sized primary matrices; though they never, I think, quite equal the very large primary matrices that may often be found taking origin directly from the substance of the pellicle. I call this "an amœboid origin" in the pellicle because these corpuscles, when they become larger than the tiniest specks, are seen to resemble embryo amœbæ in a resting stage. Their substance is pellucid with a few small granules scattered through it; and there is no appearance of their being aggregates of Bacteria such as we have invariably found to be the case with the primary matrices. No nucleus is to be seen in their interior; and they are

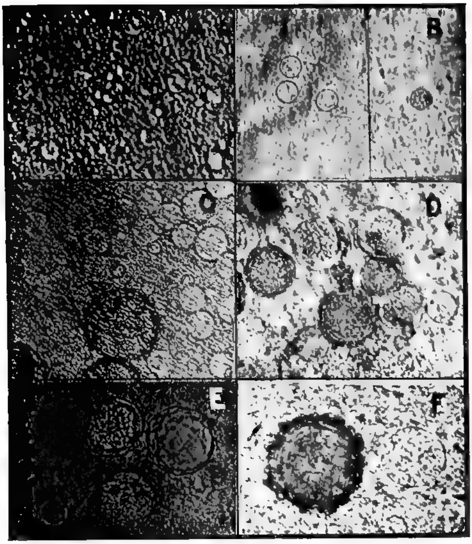

FIG. 49.

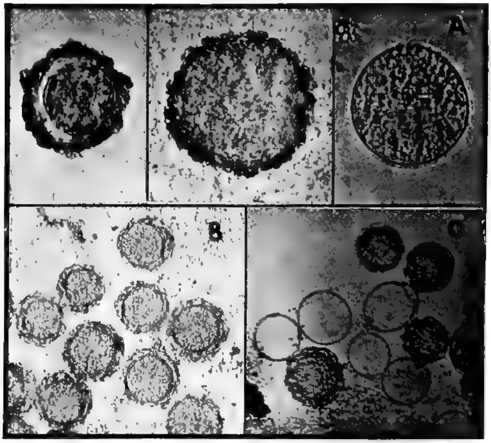

FIG. 50.

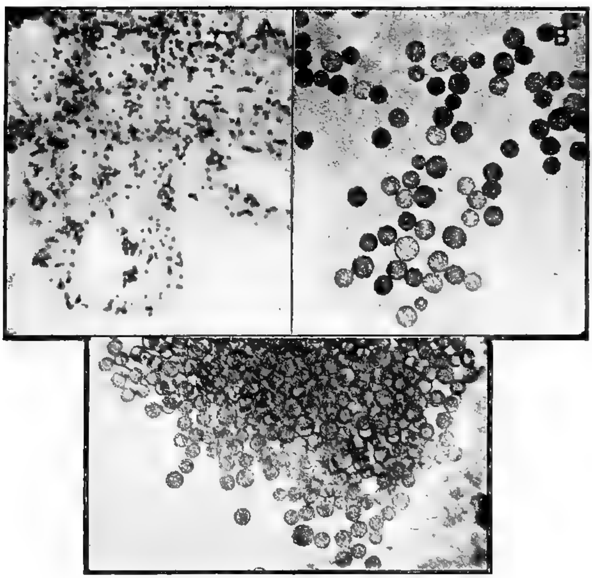

FIG. 51.

spherical and unchanging in outline, though vacuoles occasionally appear in their substance. Growth continues up to a certain point, and when it ceases, the previously diaphanous, limiting membrane becomes converted into a more or less thick-walled cyst.

In their early stages these amœboid corpuscles, destined to grow into ciliate matrices, have a very close resemblance to the discrete corpuscles, which I have previously spoken of as developing either into Monads or Amœbæ. They are to be distinguished, however, by two peculiarities, (*a*) by the fact that the embryo matrices almost invariably stain of an old-gold colour with Ehrlich's eosinophile fluid (in the proportion of three or four drops to the drachm of distilled water) while the ordinary discrete corpuscles remain unstained ; and (*b*) by the further peculiarity that they are found not only smaller but also much larger than these latter corpuscles, so that when aggregated the units may be seen to vary much in size, instead of being nearly all of one size, as the discrete corpuscles are commonly found to be.

These various peculiarities in regard to the amœboid origin of the matrices are illustrated in Fig. 49, in which A (× 500) shows many of the coming matrices in their very earliest stages, together with a few of them larger and more developed, taken from a hay infusion on the eighteenth day, in which myriads of them were appearing and growing in the pellicle. B (× 375) shows three of these bodies unstained, each of them about 1/3000 inch in diameter, from a hay infusion on the twenty-sixth day; and another of about the same size, but stained with eosinophile. C and D (× 200) show a number of these amœboid matrices of larger size, in the pellicle ; and also some cysts of the kind to which they ultimately give rise, taken from another hay infusion on the thirteenth day. E (× 375), from a hay infusion on the eleventh day, shows two of the amœboid matrices just before, and two after, encystment—one of the latter containing a revolving embryo Ciliate (as indicated in the photograph by its homogeneous rather than granular appearance). In F (× 250) an amœboid matrix just about to encyst is also shown, to the right of the thick-walled, knobby cyst, of the kind which it will speedily form. Multitudes of others were seen in all intermediate conditions.

These secondary matrices, when mature, almost always develop comparatively thick cysts, and the embryos within them sometimes remain for many days or even weeks in a motionless condition. As

a rule, also, so far as I have seen, these matrices only rarely undergo segmentation ; so that in regard to their mode of origin, their gradual growth, their formation of thick cysts, their quiescence for comparatively long periods, as well as this rarity of segmentation, the secondary matrices present characters strikingly different from those of the matrices that are developed primarily and directly from the pellicle itself—that is from aggregates of Bacteria forming therein.

I have, as a rule, seen two different kinds of cysts in these matrices of amœboid origin ; one of them being thick and brown in colour, and having the surface covered by brown, rounded knobbles. These have often been found in, or on, the under surface of old hay pellicles, somewhere between the fourteenth and the twenty-eighth days, and they vary much in size, as may be seen from Fig. 50, A (× 375), in which there is also represented an embryo, contracted under the influence of formalin, which had just escaped from another of these cysts. The second form of these cysts, and the kind which is much the more common of the two, has not the same dark brown tint, but it has a wrinkled or plicated appearance, such as may be seen under a low power in B (× 200). The formation of a cyst of this kind is common even in the segments into which the primary matrices divide, if the condition of the infusion becomes unfavourable to their existence in a free state. I have recently seen a large number of such matrices, each of whose contents divided into two active embryos ; but, conditions becoming in some way unfavourable, they speedily came to rest, and each segment assumed a spherical form within the thin original cyst. Two days later I found large numbers of these segments in plicated cysts, rather than bounded by thin, smooth membranes as they had been previously (C, × 250) ; and often, of two contained within the same cyst, one had become thus changed, while the other was still enveloped merely by the thin and smooth membrane that forms when the segment first assumes a spherical shape.

So far, I have been speaking of the origin of matrices from amœboid corpuscles in the pellicle itself, where they commonly occur intermixed with others derived directly from its substance. I now have to speak of their appearance in the villi which, in an old infusion, are apt to grow downwards from the pellicle.[1] Here,

[1] That is, where the pellicle is thick, owing to its being formed on an infusion four or five inches in depth. A thin pellicle will not develop villous growths.

in these situations, away from atmospheric influence, Ciliate matrices are formed only from amœboid corpuscles. I had often seen them swarming in such villi, and at other times had found the villi containing an abundance of the ordinary discrete corpuscles that develop into Monads or Amœbæ.

I first obtained satisfactory evidence that some of the corpuscles which appear in the villi are really embryo matrices of Kolpodæ, and then as to their rate of growth and development, during my examinations of an infusion of Dutch Clover. On the nineteenth day I found that the villi which had grown downwards from the pellicle on this infusion contained a varying number of distinct spherical amœboid corpuscles. Three days later, some of the corpuscles in other contiguous villi were found much larger, and more like embryo matrices. Two days later still, other villi were found containing a number of small but fully developed matrices, in about one half of which the embryos were slowly revolving within their cysts.

Although different villi were examined on these separate occasions, I have reason to believe, from examinations made as to the state of many others of them, at the intervals above indicated, that the rate of change observed, at three and two days respectively, fairly accorded with the average rate of development of these Ciliate matrices of amœboid origin.

The presence of these secondary matrices in the villi, often in astounding numbers, was first discovered almost accidentally. Only a very small portion of a pellicle remained on an old infusion, prepared twenty-four days previously, which had been kept in a dark cupboard the whole time, and had not been looked at for the last eleven days. Before throwing it away, I cut off a small portion of this pellicle, and, to my amazement, on examination found its villi crowded throughout with new Ciliate matrices, such as may be seen in the specimen shown in Fig. 51, A (× 15). The matrices varied much in size, and in the thickness (and consequently in depth of colour) of their brown cysts, as may be seen in B (× 60). Most of them contained embryos very slowly revolving; none of the cysts were empty, and no free Ciliates of any kind could be found in the infusion. The new brood had evidently not yet begun to emerge. C (× 60) shows how densely these cysts are massed together in some of the villi. No stain has been used for either of these specimens; the natural brown colour of the cysts causing them to appear as though they had been stained.

Some General Considerations Concerning the Origin of Ciliates in Organic Infusions.

In reference to the origin of Ciliates in filtered organic infusions there are five sets of facts of cardinal importance which must never be lost sight of. They are as follows:—

(1) It has been found that the different infusions are associated each with its own particular kind of Ciliate—that is, where such organisms appear, for it must clearly be understood that they do not show themselves in filtered organic infusions with anything like the regularity that characterises the appearance of Bacteria, or even of Monads.

(2) Then, although the infusions after their preparation had been passed through very fine filtering papers, the Ciliates which first appeared therein in swarms (somewhere between the fourth and the eighth days) were, when they first appeared as free-swimming organisms, already of nearly full size—that is, even the smallest of them (in the infusion of *Galium verum*) had seven or eight times the diameter of the largest particle capable of passing through such filtering papers; while the larger specimens (found in hay infusions) had at least twenty times the diameter of such particles. It is well known that Swedish filtering papers will not exclude particles of barium sulphate, and these, as I have found, have an average diameter of 1/15,000 inch.

(3) Ciliates are not known to be reproduced by any very minute particles having the nature of spores or germs. Lankester says[1]: "Of the formation of 'spores' in this group we are at present ignorant, in spite of all that has been written on this subject." Similarly Saville Kent writes[2]: "Among the higher orders of the Class Infusoria sporular reproduction is comparatively rare, being as yet almost unknown among the groups of the Tentaculifera, while in that of the Ciliata a few stray instances only can be cited." The "stray instances" spoken of by Saville Kent as occurring among the Ciliata are, however, quite unworthy of being considered as instances of spore-formation, seeing that what he refers to, as instances of such a process, is the fission of certain encysted Kolpodæ, Otostomata, and one or two other forms, into two, four, or eight segments—that is, into masses

[1] Introduction to translation of Gegenbauer's "Comparative Anatomy," 1878 p. ix.

[2] "Manual of the Infusoria," 1881, vol. i., p. 89.

which are often much more, and rarely less, than 1/1000 inch in diameter, and are, from the first, small Ciliates. These are the only bodies that can in any sense be regarded as "spores" among the Ciliata, which have for years been the subject of numerous and most elaborate investigations by skilled observers. Their only known modes of increase hitherto have been by processes of fission, occurring either when in their free state or when encysted; and, though much more rarely, by processes of gemmation.

(4) But it has been found that the Ciliates which first appear in our filtered organic infusions as nearly full-sized, free-swimming organisms are always preceded in the pellicle by encysted matrices from which they develop. These matrices could never, as such, have passed through the filter; and, even if they had done so, looking to their size and weight, they would have accumulated at the bottom of the vessel rather than at the surface of the fluid. They could not have got into the pellicle at all unless they, or the bodies from which they have been derived, had previously enjoyed an active life. But, as I have said, *the matrices appear in the infusions before the Ciliates.*

(5) In the face of these facts we are driven, therefore, to the conclusion that the primary matrices must be formed from the pellicle itself. And that is what I have shown to be actually the case. The early stages of these matrices have been demonstrated to be mere aggregates of Bacteria, of a kind similar to those existing in the pellicle around them. Each of these aggregates becomes enclosed by a delicate bounding membrane, develops a nucleus, and then becomes evolved into an active embryo Ciliate. This embryo may be seen revolving within its cyst, previous to rupturing it and moving away as a free-swimming organism.[1]

This mode of origin of Ciliates from the pellicle is, moreover, in exact accordance with what I have shown to be the mode of origin of small Zooglœa masses in and from the pellicle, and also with the discrete modes of origin of Monads and of Fungus-germs. We are alike incapable of explaining either set of facts, and however indisposed biologists may be, at first, to believe that these

[1] The cyst is colourless and very delicate, and there is no space between it and the embryo. But after from four to six days of free life very many of these Kolpodæ become motionless, assume a spherical shape and secrete a cyst. The body thus encysted speedily presents a totally different appearance from that of the primary matrices, seeing that its cyst though smooth is thicker and of a pale brown tint, while a narrow space exists between it and the contained organism—owing to its having undergone a slight diminution in bulk.

interesting and much-worked-at forms of life—the Ciliata—can have such astounding and comparatively rapid modes of origin, yet their origin in this way is, in reality, no more astonishing and apparently incredible than the origin of Zooglœa masses from aggregates of Bacteria, the fusion of such aggregates into individualised masses of matter, followed by repeated processes of segmentation—ultimately resulting now in the production of Fungus-germs, now of Monads, and now of Amœbæ.

Even if this last and final proof as to the actual mode of origin of the Ciliates in filtered organic infusions had not been made out, the other facts mentioned would have been, as I have said, almost impossible to be explained satisfactorily, except on the supposition that the Ciliates had been formed, in some manner, from the pellicle itself.

For if the first Ciliates that appear in infusions of hay or of other plants came from the air or the water, there ought not to be this constant association between the particular kind of Ciliate found and the material from which the infusion has been derived. And, again, if the forerunners of the Ciliates had passed through the filter as invisible germs derived from air, water, or the plants themselves, the Ciliates ought not to reveal themselves all at once (or comparatively so) as organisms of full size. All sizes between such hypothetical, invisible or minute germs and the full-sized organisms ought to be forthcoming. They are, however, absent; and, as we have seen from the previous quotations, there is no ground for supposing that any such minute germs or spores are ever formed by Ciliated Infusoria.

These difficulties taken together, as well as the others previously referred to, would be incapable of being reconciled with the facts, unless it could be shown that the Ciliates which first appear are developed in and from the substance of the pellicle itself. That being so, it becomes comparatively easy to understand the reason of particular Ciliates always appearing in particular infusions, and appearing, almost at once, nearly of full size.

So much then concerning "the direct origin of Ciliates from the pellicle"; but, as I have already shown, that is not the only way in which they arrive in organic infusions. We have also to bear in mind what I have termed "the amœboid origin of Ciliates in the pellicle." I am not altogether satisfied with this latter designation, and yet am unable at present to find a better phrase. All

that I mean to convey thereby is that the Ciliates in these cases take origin from very minute corpuscles which are, at one stage of their existence, almost indistinguishable from the discrete corpuscles that develop into Monads or Amœbæ; and that these corpuscles go on increasing in bulk, always having very much the appearance of small Amœbæ in a resting stage, till they attain varying sizes, ranging from about 1/2000 inch to 1/800 inch. They then cease growing, develop more or less thick cysts, and become converted into embryo Ciliates, which, after a time, may be seen revolving within their temporary prisons.

These amœboid corpuscles, as I have said, stain of an old-gold colour with a dilute eosinophile fluid, while the ordinary discrete corpuscles that develop into Monads or Amœbæ exhibit no such reaction. Then again, these latter corpuscles are formed from the pellicle of about the same size as the Monads and Amœbæ to which they give rise; they do not grow in bulk, they only develop. But the corpuscles that ultimately develop into Ciliates begin as very minute bodies, which I have been able to trace by the aid of stains down to 1/5000 inch in diameter; and they grow enormously, till they attain, previous to encystment, such sizes as I have above indicated.

The two modes of origin of Ciliates agree in this important respect, that in each case the Ciliates are actually formed as such within cysts; and that in each case, also, the free-swimming organisms emerge from the cysts approximately of full size.

As to the actual source or origin of these minute particles which appear after many days in the midst of the pellicle or of its outgrowths, and go on increasing in size till they become encysted masses, each of which gives origin to a Ciliate, nothing definite can at present be said. It may be surmised by some that they are germs or minute spores derived from some previous Ciliates. But against such a supposition various weighty considerations may be urged.

In the first place, as we have seen, no such germs or spores are known, so that the supposition would be opposed to all existing knowledge.

Then, there is the fact that these bodies only show themselves after ten or fifteen days; and even then only when we have to do with infusions four or five inches in depth, or so strong as to be capable of forming thick pellicles from whose under-surface numerous villous outgrowths project. In such zooglœal out-

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growths, or in the substance of the thick pellicle itself, these minute Amœboid bodies appear in myriads and go on to the production of Ciliate matrices such as we see in Figs. 49, 50, and 51; while in a weak or a shallow infusion, forming only a thin pellicle, no such bodies present themselves.

Then, again, in the cases where these minute Amœboid bodies arise and go on to the formation of Ciliate matrices, is it conceivable that myriads of them, always of precisely the same kind, should occur in each particular infusion if they were due to external contamination? It cannot be supposed that their vast numbers are due to self-multiplication; for such particles are motionless, and through their various stages of growth can never be seen to divide till they have attained their full size and have become converted within their cysts into embryo Ciliates.

There is the further important fact, telling strongly against the origin either of the primary or of the secondary forms from external contamination, that Ciliates only appear in filtered infusions prepared from ripe or from dead grasses, and not from immature grasses in the fresh state, as I have shown elsewhere.[1] If contamination came from earth, or air, there seems absolutely no reason why it should not come with fresh, immature grasses, as well as from ripe, dead grasses.

A striking illustration of this kind of difference has again been met with quite recently. On June 14th I gathered some of the Common Holcus (*H. lanatus*), which was just beginning to flower, cutting each stalk about two inches from the ground. On reaching home, within an hour, I cut up some of the stalks and leaves and made an infusion in the ordinary way, except that I used distilled instead of tap water. After macerating for four hours at about 80° F. the infusion was filtered through two layers of fine Swedish paper, and placed under a bell-jar. After three days the turbidity was only slight, and the scum on the surface was scarcely appreciable: it was very thin and non-coherent even on the fifth day, and showed, on examination, nothing but Bacteria. The scum was examined again on the tenth and on the fourteenth days, and was found on each occasion to be thin and to contain only different kinds of Bacteria: not a single Zooglœa mass, Monad, Amœba or Ciliate was found on either occasion.

On June 22nd another similar infusion was made with distilled water from the stalks and leaves of some of the same grasses, which, during the intervening eight days, had been lying on a newspaper within a closed drawer. Everything was the same, therefore, except that the infusion was made from grasses which instead of being fresh had been *dead* for eight days. The infusion was filtered in the same way, and then placed beneath the bell-jar by the side of the other. After thirty-six hours this second infusion was already much more

[1] "Studies in Heterogenesis," p. 87.

turbid than the first (then ten days old), and the scum on its surface was thicker, more coherent, and found to contain, in addition to many different kinds of Bacteria, a multitude of minute Zooglœa masses in a very early stage. When examined again on the sixth day, the turbidity was more marked and the pellicle was thicker; and the first portion examined microscopically was found to be swarming with Zooglœa masses segmenting into Monads, together with discrete corpuscles and multitudes of active Monads. There were swarms, also, of the primary Ciliate matrices in the pellicle, together with large numbers of free and active Kolpodæ. But examination of the scum made from the fresh grass, on this fourteenth day showed it to be still very thin, and composed only of Bacteria. No single Zooglœa mass, Monad or Ciliate, was seen in the portions of this pellicle that were examined.

The conclusions to which the evidence compels us to arrive seem almost incredible. It was amazing enough to find aggregates of Bacteria fusing and undergoing molecular changes, as a result of which now Monads, now Amœbæ, now Fungus-germs appear as ultimate products; but when we find larger masses of Bacteria individualising themselves and going through a series of inscrutable changes till revolving Ciliated embryos are produced, which speedily rupture their cysts and appear as active free-swimming animalcules, there is room for still greater amazement. Still the evidence seems absolutely conclusive; and in face of it our preconceptions must give way.

We are likewise unable to account, by means of known facts, for the late secondary brood of Ciliates that has been traced as beginning in minute particles which gradually increase in size, and ultimately produce very similar encysted matrices, from which Ciliates are born. Existing knowledge can give no explanation of the origin of such bodies. Are the particles from which they proceed minute germs of Ciliates? Science knows nothing of such germs. Are the particles then only another primordial form of life, having the potentiality of unfolding, as they grow, into Ciliated Infusoria? This seems incredible: yet if they are not unknown germs, they must be new-born particles.

(b) On the Transformation of Encysted Euglenæ into Ciliated Infusoria.

Many different forms of Ciliates are at times produced by the transformation of encysted Euglenæ kept under unnatura conditions. When, for instance, a gathering of Euglenæ is put into a rather shallow vessel of water and left exposed to light and air

which they were converted into flagellate Monads or Peranemata have been already described (pp. 221–224); but the question of their origin was deferred. All that was said on the subject of the origin of these spinous cysts was this: "I will only say that I have found many thousands of them in association with a gathering of Euglenæ; and though I have seen many specimens of Stylonychiæ come out of them I have never seen a single one of them formed by the encystment of such a Ciliate. They appear rather to have been derived from much smaller matrices (found in association with the more developed specimens) which gradually increased in size and formed a thicker envelope, upon which the spinous processes were finally developed. The mature specimens varied a good deal in actual size."

Such bodies as are to be seen in Fig. 52, A (× 80), when they have become slightly increased in size and show indications of a nucleus, are represented, under a higher power, in B (× 250). In C (× 250) two of these bodies are shown which have still further increased in size, and the one on the right may be seen to have begun to develop spines from the outer surface of its cyst. Such spines, fully developed, are shown in D (× 250), in which the surface of one of these cysts has been focussed. E (× 250) represents another of these bodies whose contained embryo had just begun slowly to revolve within its cyst. When I proceeded to photograph it no movements were appreciable, but these must have been stimulated under the strong light from the lamp, the result being the homogeneous appearance shown in the photograph. The organism that has many times been seen emerging from such a cyst, after a series of violent movements, has been an embryo Stylonychia of almost full size.

In other cases, the transformation into Ciliates of Euglenæ that come to rest on the surface of the fluid, and within cysts so thin as to be scarcely appreciable, has been seen taking place much more rapidly. I have traced all the stages in the formation of Nassulæ in this way; and will now describe and illustrate the mode in which the transformation has been effected.

Towards the end of November, 1901, after two days of slight frost, I procured from a ditch some damp mud on which there was a coating of Euglenæ. A portion of this was placed in a small vase, which was then filled with water. The vase was left on the end of the mantelpiece nearest the window in my study, covered

by a glass shade, which was allowed to overlap the edge of the mantelpiece, so as to permit access of more air to the Euglenæ than they would otherwise have had.

When examined three days later, a thin scum, composed in the main of small Euglenæ, was found on the surface of the fluid. A few of these organisms were seen to be becoming decolourised, and exhibiting different stages of the process. They were all motionless, round or oval, and of just the same varying size as the unaltered Euglenæ among which they were situated. During my examination of this pellicle, many flagellate Monads and Rotifers were seen ; but no Ciliates of any kind were met with. Two days later the decolourised Euglenæ were found to be still more numerous, while many of the unaltered Euglenæ were slowly rotating within their thin and scarcely visible cysts. Twenty-four hours later, that is, on the sixth day after gathering the Euglenæ, I found a few of the decolourised specimens showing traces of the formation of the characteristic buccal cylinder of a Nassula, and a few days later I saw such Ciliates revolving within, and issuing from, their cysts.

These several stages of the transformation may be illustrated by the photographs comprised in Fig. 53. Thus, a group of five Euglenæ in different stages of decolourisation is shown in A (× 375). In the two lower specimens the change is most advanced, and in the one on the left it was complete except for one small mass of the green substance. B (× 375) shows three other Euglenæ completely decolourised, except for a few green granules remaining, together with a vestige of the red eye-speck in the upper specimen. That on the right shows a small vacuole ; but in neither of them is any trace of the buccal tube or of a nucleus to be seen. In C (× 375) the first rudiments of the buccal tube are to be seen appearing in two of these transformed Euglenæ, and in another specimen, in which the tube seemed to be in about the same stage of development, an ovoid nucleus was seen, though I did not succeed in photographing it satisfactorily. D (× 375) shows one of these Ciliates, which was killed by a very dilute formalin solution, just after its emergence from one of the cysts. It seems large in proportion to the size of the cyst from which it has escaped—as is generally the case both with Ciliates and with Rotifers. It bears, however, the usual marks of immature development. This is indicated by its delicate texture, and by the scanty development of a few weak Cilia about its anterior

extremity only, as well as by the comparative absence of food contents—so that the nucleus and the more developed buccal cylinder are very plainly seen. In twenty-four hours or so the appearance of the organism becomes distinctly altered, as may be seen by the specimen shown in E (× 375), in which short Cilia had developed all round the body, and in which the larger amount of contents hid the nucleus and in part also the buccal cylinder. Later on, when such organisms become fully developed, they prove the most ravenous creatures, and may often be seen gorged with food of different kinds—sometimes Diatoms, sometimes Oscillatoriæ (which may be taken in so as greatly to distort their bodies), and at other times with green algoid corpuscles, such as may be seen in F under a lower degree of enlargement (× 250).

On other occasions I have seen Chilodons produced from Euglenæ by a very similar set of changes. Transformations of this kind, as well as into other forms of Ciliates, or into Amœbæ, are prone to occur when portions of a fresh Euglena pellicle are transferred to the surface of water in a stone pot, on which the cover is subsequently placed. On examination of portions of this pellicle after an interval of seven, ten, or more days, we may often find transformations, of the kind indicated, occurring in some of the Euglenæ which have thus been left not only cut off from all rays of light, but in a very confined air space.

(c) On the Segmentation of some Encysted Amœbæ and the Conversion of the Segments into Ciliated Infusoria.

It has been known since the investigations of Spallanzani that tufts of moss and lichen are tenanted by three kinds of organisms, all of which have the power of reviving after even prolonged periods of what appears to be complete dessication. The animals recognised by him were certain Rotifers, a few Nematoids, and some of the curious group of Tardigrades, to which Spallanzani gave the name of 'Sloths.' When working at the 'Free Nematoids' in 1864 I repeatedly found these same organisms in moss and lichen from the most varied sites, and ascertained that the Nematoids met with, in most cases, belonged to one or other of two genera (Plectus and Aphelenchus), several species of which were described in my "Monograph on the Anguillulidæ

out coarsely-lobate projections as in Fig. 54, A (× 150). These projections are generally perfectly hyaline and translucent, contrasting notably with the blood-red colour of the great bulk of the organism. Sometimes large quantities of undigested refuse, in the form of spherical pellets of different sizes, are extruded, and the Amœba may begin to encyst itself at once, as in the paler red specimen represented in B (× 200). The movements of these creatures are so extremely sluggish that I was able to take the foregoing photographs while they were still living.

These Amœbæ are not always of a pale red or dark red colour; some are nearly colourless. Such variations depend upon the portions of the lichen upon which they have been feeding—some of the under surface being whitish, while other parts are of a red-brown colour. The undigested refuse pellets may be all extruded before, or not till after, the Amœbæ have begun to encyst themselves. The cyst itself is extremely thin and apparently somewhat glutinous, as particles of different kinds and some of the small refuse pellets are generally found adhering to its outer surface, as in Fig. 54, B. In this specimen digestion had only been partially accomplished, as there were food-masses still within, as well as outside, the pale red body-substance.

One of these pale red encysted Amœbæ was found which had divided into eight or ten unequal spherical segments, such as may be seen in C (× 200). Only five segments are distinctly to be made out in the photograph, but under examination by the microscope others were seen at lower levels. They were all motionless, granular masses of protoplasm—probably about to be evolved into Ciliates, seeing that such bodies densely packed with large pale red corpuscles and granules were seen actively swarming about within other of these Amœba cysts. One of them containing six dark red and very active Ciliates is shown in D (× 200), after the movements of the Ciliates had been brought to rest by means of a weak osmic acid solution. Both of these cysts had an abundance of the usual minute particles adhering to their outer surface. Another rather larger cyst, containing six very active, blood-red Ciliates, is shown in E (× 200). This cyst is seemingly dilated by the active movements of the Ciliates, some of which had probably escaped through a small opening below, where the thin membrane can be seen to be bulged. Other similar specimens have also been found containing the same kind of Ciliates, more or less red, and the flexible cysts were coated externally, as were

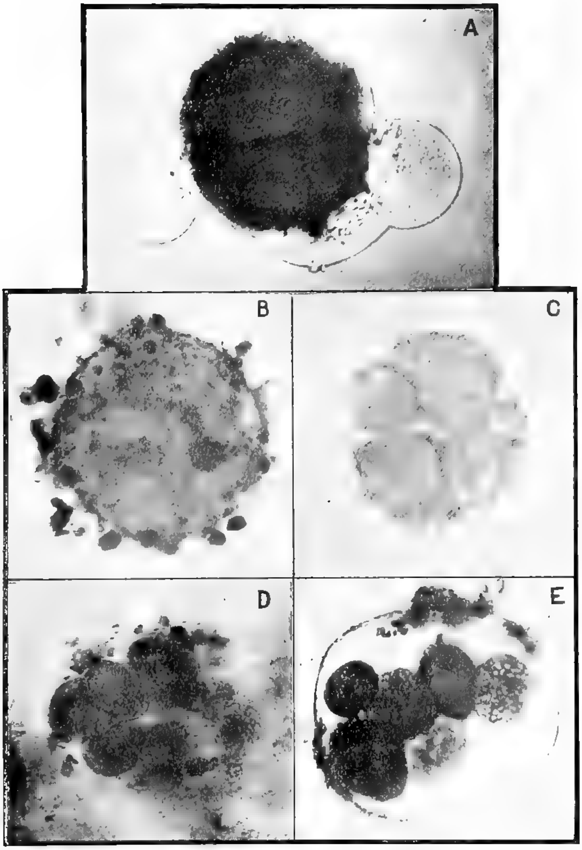


Fig. 54.

To face p. 250.

all the great encysted Amœbæ, with a large quantity of adherent foreign particles.

These red Ciliates, in their immature condition, as found within the cysts, are slightly pointed at the anterior extremity, where short cilia are principally developed. Their bodies have been so closely packed with red corpuscles[1] that I have hitherto quite failed to make out the shape and situation of their mouth; and I have been unable to preserve them long enough in their free state beneath the cover-glass to allow these corpuscles to disappear. They doubtless would disappear in the course of a day or two, just as the very similar corpuscles found in the bodies of immature Glaucomas, derived from the eggs of certain Rotifers belonging to the genus Callidina, have been seen to disappear during the time needful for the attainment of their adult form, as I have elsewhere described.[2]

We have here, then, another very important mode of origin of Ciliates, of a kind not previously suspected. The orange-red colour, more or less marked, of the Amœba in its free and in its encysted state; the character of its cyst; the finding within such a cyst a number of motionless but unequal red spheres, into which the encysted mass had separated; and within others the existence of a number of similarly unequal, active, red Ciliates, constitute a set of facts which can only admit of one interpretation. It seems clear that the red Ciliates have been developed from the red Amœbæ, after they had become encysted and had undergone segmentation—under the influence of the unnatural conditions to which they were exposed during the soaking of the lichen for many days in distilled water.

(d) On the Transformation of the Substance of the Eggs of a Tardigrade into Ciliated Infusoria.

The Tardigrades, on which the following observations have been made, have all belonged to the genus Macrobiotus, and on reference to the plates and descriptions given by Doyère in his celebrated "Mémoire sur les Tardigrades"[3] it is clear that they have all been specimens of *M. Oberhäuser.* These curious creatures were first observed and described by Eichhorn in 1767, and named by

[1] The mode of formation of which is referred to on p. 268.
[2] "Studies in Heterogenesis," pp. 119–124.
[3] "Ann. des Sciences Naturelles" (Zool.), 1840, p. 269.

him 'Wasserbären' or Water-bears; while later on they were termed 'Sloths' by Spallanzani, who devoted much attention to their tenacity of life and power of recovery after long periods of dessication in tufts of moss and lichen.

The specimens that I have had under observation have been found in association with the great Amœbæ referred to in the last section, together with certain Rotifers, obtained from portions of a yellow lichen (*Parmelia parietina*) brought from Bagnières de Luchon seven months previously, after they had been soaked in distilled water from seven to ten days. The fragments of lichen were allowed to soak for this long period (the weather being cold) because in this way, as I have previously indicated, many more of the specimens of Amœbæ, Rotifers and 'Sloths' dropped away from the lichen and were obtainable than would have been the case if the soaking had only been for the few hours needful to enable these and other organisms existing in the lichen to resume their active life.

A drop of the fluid, left after this soaking, together with some of the sediment, was transferred by a tiny pipette to a microscope slip, and on each side was placed a minute fragment of one of the thinnest cover-glasses, so as to protect the organisms from pressure and allow them to move about after a cover-glass has been applied. Every portion of this drop was then thoroughly scrutinised under the microscope, with the result that occasionally one, two, or even three specimens of Macrobiotus would be found—young or old, living or dead.

They are of a red-brown colour, owing to the presence of much pigment of that tint in different parts of the body, but especially in a broad band of pigment cells lining the inner side of the integument along the middle and lateral regions of the back. This superabundance of pigment is one of the distinguishing features of *M. Oberhäuser.* It is more plentiful in some individuals than in others, and especially in old specimens. Many of them contained great dark-coloured eggs, varying from two to five in number, such as are shown in Figs. 55, A, 56, A.

My attention was specially directed to these creatures owing to the following circumstance. I was at the time studying the development of Ciliates from the eggs of Callidinæ, as described elsewhere.[1] I placed, on March 18, 1902, a slip containing

[1] "Studies in Heterogenesis," pp. 119-124, Pl. IX., figs. 95-98. The Ciliates produced from these Rotifers belonged to the genus Glaucoma.

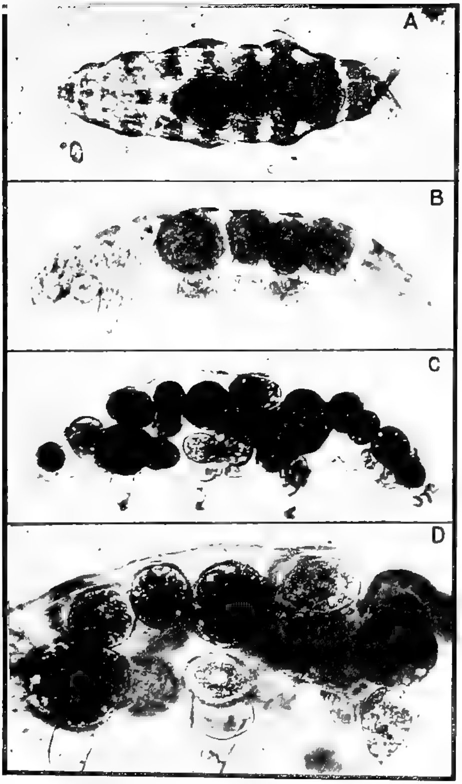
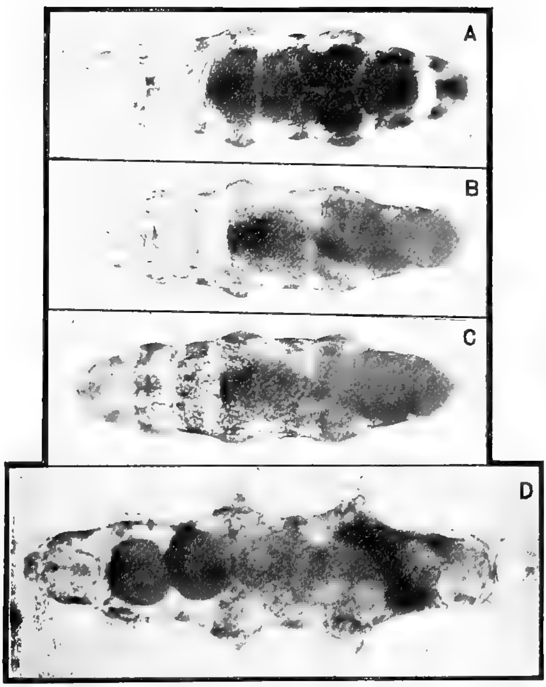


FIG. 55.



specimens of these Rotifers mounted in the manner I have described, in a damp chamber on the mantelpiece of my study. Among them was found a young dead Macrobiotus, containing two eggs. This microscope slip was examined every two or three days, and a little more distilled water was added to that beneath the cover-glass if it had begun to evaporate. The single Macrobiotus existing in this drop of fluid was seen on March 22 to be seemingly unaltered, but no photograph was taken as I was specially watching other objects beneath the same cover-glass. It was not again examined till March 25, when to my great surprise I found within the integuments of the 'Sloth' eight very slowly-moving Ciliates, filled with red-brown corpuscles, together with five pale, merely granular, spherical masses. Nothing of the animal was left within the integument save the pharynx and the suction bulb—not even the pigmentary lining: that had evidently been swallowed by the Ciliates. The integument itself seemed whole and entire, so that this astonishing transformation, which had taken place within the space of three days, greatly piqued my curiosity.

Subsequently, therefore, I sought for specimens of Macrobiotus containing eggs, in drops of the fluid and sediment mounted in the way I have mentioned, and will now relate what was seen in two other individuals.

On April 20, a dead Macrobiotus containing four closely approximate and rather irregularly shaped eggs was found in a drop of the water and sediment just mounted for examination. The specimen was not photographed at the time, but it was very much like that shown in Fig. 55, A (× 115).[1] It was associated with other organisms which I desired to keep under observation for a time, so that the microscope slip containing it was placed, as usual, in one of the before-mentioned damp chambers. On April 24 I found the Macrobiotus in the altered condition shown in B (× 150). Its four eggs were no longer closely approximated, so far as the first and second were concerned; each of them had become more or less completely spherical; and there were now distinct indications within of the formation of globules or vesicles, such as were found in the Ciliates derived from the great red Amœbæ, and such as we shall find further on in a great Ciliate

[1] This specimen was intended to have been photographed like others with an enlargement of 150 diameters, but by mistake a wrong eye-piece was used—hence its smaller size.

derived from a transformation of the egg of a Rotifer (Hydatina). Recognising its condition I made only a hasty examination, and abstained from taking a photograph at a higher power than 150 diameters, in order to avoid the chance of interfering, by a more prolonged exposure to the light of the lamp, with the progress of changes that seemed to be taking place in the eggs. I much regretted this at the time, epecially because of the appearance of the anterior of the four eggs, in which the formation of corpuscles, such as are found in the Ciliates, appeared to be going on, though in their ordinary condition the eggs of the 'Sloth' are, like those of Hydatina, simply granular. On examination after twenty-four hours, I again found that a complete metamorphosis had taken place ; the four eggs were gone, and with them all that was within the integument of the Macrobiotus, including the whole of its scattered pigmentary lining, and in their place were found about twenty very active red-brown Ciliates of different sizes, such as are shown in Fig. 55, C (× 150), after they had been killed by a weak osmic acid solution. The bodies of most of them were filled with the usual great corpuscles ; some of them were distinctly pyriform in shape, and cilia were principally seen about the narrow anterior extremity—indicative of their embryonic character. A portion of this 'Sloth,' more highly magnified, is to be seen in D (× 250), which shows the great corpuscles within some of the Ciliates, and the pyriform shape of others.

Looking to the previously altered shape and appearance of the eggs, as shown in B, and to what was found only twenty-four hours later in C, the most feasible explanation of this transformation, after all that I had previously seen and some other things that will be described in the present chapter, is (*a*) that the eggs had been converted into great Ciliates ; and that subsequently two processes went on, (*b*) the gradual devouring by these creatures of the body substance of the parent organism, and (*c*) the multiplication by fission of the Ciliates, as they gorged and grew.

Happily a more recent observation has enabled me to prove the actual occurrence of the first of these processes—namely, the conversion of the egg of the 'Sloth' into a single great Ciliate. The following details and illustrations will make this clear.

On May 9, a rather large Macrobiotus, with five large eggs, was found dead beneath a cover-glass, though it had been seen alive and with the same number of eggs, only twenty-four hours

previously. This creature is shown, looked at from above and with the eggs in focus, in Fig. 56, A (× 150). It will be seen that instead of four eggs in linear series, the last but one is replaced by two eggs, side by side. There was no other 'Sloth' beneath this cover-glass, and fortunately it was lying alone, apart from other matter or organisms. After photographing the specimen, the microscope slip on which it was contained was replaced in the damp chamber. Three days later, at midnight, before putting away my work, I fortunately examined this specimen again, and found, as I had done on the previous day, that the two anterior eggs had become much altered and in part disintegrated—their lining membrane, and part of their contents, had apparently disappeared, so that there was a light, single granular mass in the situation which the two eggs had previously occupied. Now, also, the hinder egg was seen occupying more of the posterior extremity of the animal; and attentive examination showed that the whole mass was very slowly—almost imperceptibly—revolving within a limiting membrane, and occasionally a flicker of cilia could be seen. It was likewise clear that the whole mass was now largely composed of small corpuscles such as seem to be forming in the anterior egg shown in Fig. 55, B. As soon as these facts were clearly made out I photographed the specimen with a brief exposure, and the result is shown in Fig. 56, B (× 150).

The slip was then replaced in the damp chamber with the addition of a little more distilled water to the edge of the cover-glass, and bearing in mind the rapidity with which the changes had taken place in two other specimens of Macrobiotus, I determined to examine this specimen again after two hours. Accordingly, at 2 a.m. on May 13 I again put the specimen under the microscope and then found that there were two masses revolving, and rather more quickly, instead of one—the new Ciliate being situated anteriorly and partly concealing that which was first seen. This is only obscurely indicated by the photograph then taken, a reproduction of which is shown in C (× 150).

No examination was made again till after the expiration of seven hours, and the condition then found is shown in D (× 200). The two great Ciliates had by this time got into the anterior part of the body, and were revolving more rapidly while the other three eggs were disintegrated. As I did not wish to kill the Ciliates at this stage I took a photograph of the specimen at a

low power (× 65) and with a short exposure, before returning the slip to the damp chamber.[1]

I examined the specimen again after another seven hours, and still found only two Ciliates, though they had grown, and were much more active. Evidence of their previous activity was also afforded by the disordered and fragmentary condition of the remains of the other eggs, and of the internal organs of the 'Sloth.' It was impossible to photograph the Ciliates again in their more active condition, and in my attempt to remove the Macrobiotus from underneath the cover-glass, where there was another specimen that I much wished to preserve, I unfortunately lost it.

After all, however, the evidence that was wanted had been supplied—the egg of the Macrobiotus had been proved to be converted into a Ciliate. *The large stationary egg had become replaced by a slowly moving body of like size, which subsequently proved to be a great Ciliate*, and no smaller Ciliate was ever seen within the body of this animal. The other two steps of the process by which in twenty-four hours such a specimen as is shown in Fig. 55, B, is enabled to yield the appearance represented in C is plain and easy to be recognised—rapid fission of the Ciliate or Ciliates, together with their voracity, will account for all that C represents.

There is the further noteworthy fact that in various specimens of Macrobiotus that have been under examination, in which there were no eggs, no Ciliates have ever appeared.

In the specimens shown in Fig. 56 the changes had been very slow in comparison with those shown in Fig. 55—seeing that after sixteen hours there were in the former specimen still only two Ciliates. This I am disposed to think was probably due to this specimen having been examined three times and photographed three times, while the changes were in progress, instead of only once, as for the specimen whose changes are shown in Fig. 55.

Still, the restrictive influence of light over these heterogenetic changes occurring in the eggs of Macrobiotus, is altogether slight as compared with the absolute stop which exposure to light puts to the heterogenetic changes occurring in the eggs of Hydatina which I shall presently have to describe.

Whether these same changes in the eggs of Macrobiotus would

[1] This photograph has since been enlarged to its present size.

take place in the dark or not, as well as the question of the genus to which the Ciliates which are thus originated belong, must for the present be left undetermined, as my supply of the lichen from which these particular 'Sloths' were derived was exhausted.[1]

These changes in the eggs of the Macrobiotus are very interesting in another respect—seeing that they will take place beneath a cover-glass, though it is extremely difficult to obtain evidence that the closely-allied heterogenetic changes which I have seen occurring in the eggs of certain Rotifers will occur under similar conditions.

That we have here again to do with a definite process of Heterogenesis seems perfectly clear, as a consideration of the following facts will show :—

(1) The Macrobiotus has only a small suctorial mouth, and the general envelope of its body is tough and resistant.

(2) No Ciliates can ever be seen attempting to penetrate from without.

(3) The Ciliates have only been found in female 'Sloths' which had previously contained eggs.

(4) The Ciliates, as first seen, are large but slowly-moving embryos of the same size as the eggs—like those that were found in the specimen represented in Fig. 56.

(5) It would be quite impossible for large embryos of this kind to penetrate from without, through the tough integument of the 'Sloths.'

[1] In the following year I tried to repeat these observations, and again brought some of the same kind of Lichen from the Pyrenees, though from another locality. Unfortunately, however, it contained only a very few immature specimens of this 'Sloth.' Not a single one containing eggs was found. Another species, *M. Hüflandii*, has since been found in a pond at Northwood in association with masses of Oscillatoriæ. These creatures contained plenty of eggs, and 15–20 were occasionally seen in their cast-off coats. Though a few trials were made with such eggs, none of them went through changes similar to those just described.

CHAPTER XIII

THE HETEROGENETIC ORIGIN OF CILIATED INFUSORIA (CONTINUED)

(e) On the Transformation of the Immature Eggs of a Gnat-like Fly into Ciliated Infusoria.

WHILE studying the changes that occur in the scum on an egg and water emulsion, in the summer of 1900, I occasionally found on the surface of the fluid, within thirty-six hours of its being prepared, a small grey fly with monilated, hairy antennæ, which, as I subsequently learned, belonged to the genus Psychoda. On July 4 of that year I had the curiosity to examine under the microscope one of these flies which had died on the fluid. I put it into a drop of water, placed a cover-glass over it in the ordinary way, and on examination found that the weight of the latter had ruptured the abdomen, and that a great swarm of minute spherical or ovoidal Ciliated Infusoria were pouring out from the abdominal cavity, while an equally large number were to be seen swarming about within the abdomen, mixed with other bodies apparently similar except that they were motionless. Fig. 57 shows a few of the Ciliates, which emerged from the abdomen, after they had been killed by a weak solution of iodine. One, near the centre, may be seen to be very much larger than the others.

Each summer since, during the month of July, except in 1903 when that month was unusually wet and cold, I have found other specimens of these flies and many of them have contained the same kind of Ciliates—sometimes as many as two or three hundred within a single fly. The flies have only appeared on the fresh egg emulsion when the temperature of the air has been for some days at or above 70° F., and they have been found on the emulsion long before any Ciliates appeared therein.[1] The flies containing the

[1] Sometimes I have only been able to obtain specimens towards the end of July, on account of the weather being unfavourable, and as during August I have

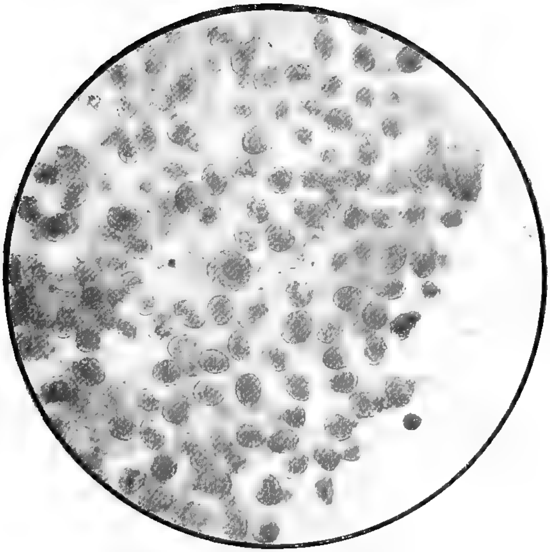


FIG. 57.

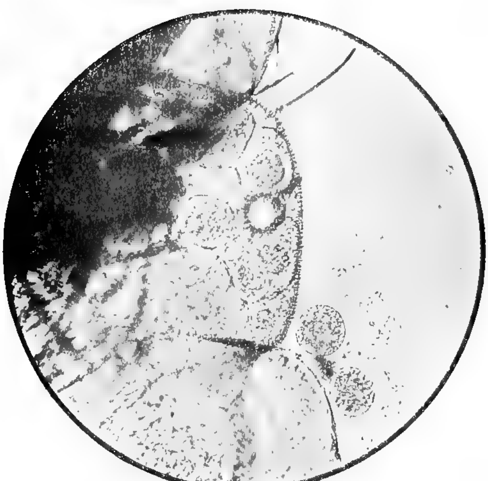


FIG. 58.

To face p. 258.

Ciliates have been found to belong to two species, *P. phalœnoides* and *P. sexpunctata*, as well as another of the same genus but rather larger, and with a narrower abdomen marked by more obvious bands than in either of the other two. They have been met with most frequently in *P. phalœnoides*, and *always only in the females of each variety.* That no Ciliate has ever been seen within the abdomen of a male fly, of either of these species, will be found to be a point of great significance.

Examination of the flies soon showed that they possessed no recognisable alimentary canal. Like their allies, the Harlequin Fly and the Ephemeridæ, these Psychodæ do not feed, and their brief life in the imago state seems solely devoted to the processes of reproduction.[2] When the mature eggs in some, or the Ciliates in others, have been voided, little is to be seen within the abdomen, apart from the vascular system and the air tubes, save a varying number of fatty processes attached to the abdominal wall, and the terminal portions of two brown Malphigian tubules.

At first I was extremely puzzled to account for the presence of these swarms of Ciliates in some of the flies—especially seeing that they were flies which did not feed and possessed only the abortive rudiments of an alimentary canal. The fact that the Ciliates were found only in female flies (which were also very much more common than the males) was an important point, and soon became all the more important when, on several occasions, I discovered a number of loose immature eggs of various sizes, scattered about among the Ciliates, in different parts of the abdomen.

What the meaning of these immature eggs being loose in the abdominal cavity may be, and how this condition of things is brought about, I am unable to say; but a careful examination of such eggs, and of the various states of the embryo Ciliates to which I am now about to refer, has forced me to come to the conclusion that the latter have been derived from the former.

In Fig. 59, A (× 250) one of the larger of the specimens of these immature eggs is to be seen, while in B (× 375) one of the very

been away, my supplies have been fitful and not very abundant. Some odour appears to attract the flies to this mixture, possibly in connection with their egg-laying instinct.

[2] In their work "On the Harlequin Fly," 1900, p. 106, Miall and Hammond say, "It is evident that Chironomus does not feed in the winged state. The mouth parts, though of elaborate structure, are never used in feeding, and the alimentary canal of the fly is empty, except for a greenish fluid, which fills the stomach of the pupa and newly emerged fly."

large motionless bodies otherwise similar to the Ciliates is shown, such as may also be seen near the centre of Fig. 57. Although this particular body was motionless, others, just as large, have been seen slowly moving by means of a few short, languidly-playing cilia. This body possessed a very large, finely granular nucleus, like that in the egg: proportionately larger, it is true, and otherwise differing in appearance from the one being unstained and the other stained. But exactly similar very large nuclei are to be seen in the smaller Ciliates shown in Fig. 60, A, B ($\times$ 375), also stained by a weak solution of iodine, which were moving slowly by very short delicate cilia, such as are dimly indicated in the figures. Some of these smaller bodies were motionless; while others were seen slowly revolving within a most delicate hyaline cyst. This was the condition of the two bodies that are shown outside the abdomen of the fly in Fig. 58. The application of the iodine solution caused rather wild movements for a moment, which dilated the cysts. These are still faintly indicated in the figure—that around the lower of the two Ciliates being the more distinct. Within the portion of the abdomen shown in the figure seven other and rather larger Ciliates, with great nuclei, are represented.

The large body shown in Fig. 59, B, is, I believe, an encysted matrix, destined to develop cilia and to undergo fission after emerging from its hyaline cyst, so as to produce such smaller embryo Ciliates as are shown in Fig. 60, A and B. That they are truly embryos, beginning their active life as bodies of this size, is confirmed by the following facts.

Some of these Ciliates have been kept alive on two or three occasions in dilute egg emulsion for twenty-four hours, and by that time these extremely delicate bodies had undergone a very distinct development. Their investing membrane had become firmer, and the cilia larger and much more numerous, as shown in Fig. 60, C ($\times$ 375); while the bodies of the Infusoria had become densely packed with granular matter, which almost completely hid the nucleus. In some of them, as in this particular specimen, a sluggish contractile vesicle was seen, but I have not been able to recognise the mouth in any of them. Their movements of translation were rapid, and associated with partial rotations of the body. On two separate occasions, when they have been kept alive for twenty-four hours, I have found large numbers of them with short abortive cilia and in a state of partial division, as shown in D ($\times$ 375); though, strangely enough, in none of them

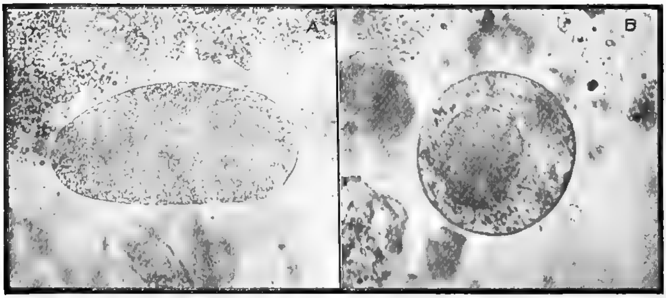


Fig. 59.

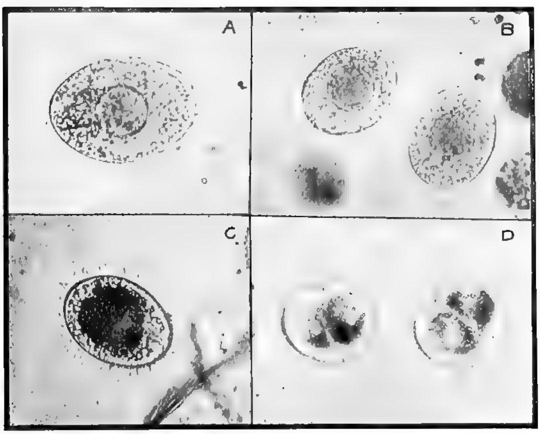


Fig. 60.

To face p. 260.

has the division been further advanced than it is represented as being in these specimens.[1]

I am disposed to think that these Ciliates would develop into Glaucomas, in which the mouth is very small, and often rather difficult of detection. Their general shape and the very large size of the nucleus is in accord with this supposition. Still, although the nucleus in a Glaucoma is often very large and spherical, in these embryo Ciliates it is far greater in proportion to the size of the body than in any other Ciliates I have ever seen.

It is perfectly clear, however, that the hundreds of Ciliates to be found in the fly are extremely delicate embryos, as shown by their texture and ready disintegration,[2] and also by the change that takes place in them and in the development of their cilia in the course of twenty-four hours. That such large and frail embryos could in any way penetrate the body of the fly from without (even if the stage with languid movements and short weak cilia were their first stage) would have to be regarded as almost impossible. But, as we have seen, in their first stage they are really *motionless bodies contained within hyaline cysts*, and there is no possible source for such bodies, in such numbers, except from the immature ova, to which they are closely allied in structure.

This resemblance was seen by an examination of the loose immature ova within the abdominal cavity, as well as of those that were observed within portions of the ovaries which had been extruded from the abdomen of other flies. And in one of these flies, where glycerine had been allowed to run in under the cover-glass, so as gradually to replace the distilled water, I found six days later, that the nuclei of the immature ova and also those of the Ciliates had assumed a pale bluish tint—so that the nuclei in each case presented an altogether similar appearance.

The normal development of eggs seems to take place very rapidly in these Psychodæ, judging from what I have seen in one of them. A living specimen of *P. phalænoides* was found containing

[1] One of the flies which had been ascertained to contain hundreds of these Ciliates, was left in the egg-emulsion for twenty-four hours, in the expectation that the abdomen would be ruptured by its swarming brood and the Ciliates liberated. But this did not happen, and on examination I found the whole body of the fly densely packed with motionless and dead Ciliates.

[2] In one case all of the Ciliates which had been left in water under a cover-glass were found to be dead in four hours; and on another occasion others, under similar conditions in a damp chamber, were found after ten hours not only dead but almost completely disintegrated.

what appeared to be a small number of half developed eggs. It was replaced on the egg and water mixture, and examined again after twelve hours. The fly was then dead, but its abdomen was greatly distended and quite opaque owing to the presence of a very large number of ova. The first fifteen that were expressed were well developed ; the others less so. It contained no Ciliates.

What the particular conditions are that lead to the premature discharge of immature eggs into the abdominal cavity remains altogether obscure and unknown. This, however, I am disposed to think is the first event ; and the second appears to be some inscrutable molecular changes occurring in these immature eggs, whereby they become converted into Ciliated Infusoria. So far we have to do with suppositions : but the actual facts may be summarised as follows :—

(1) In female flies that do not feed, specimens are occasionally found in which a number of immature eggs of varying size are found loose in the abdominal cavity, mixed with a large number of embryonic Ciliated Infusoria, some of which are very much larger than others, together with motionless bodies otherwise similar, and similarly varying in size.

(2) All these bodies, but especially the last two varieties, are notable for the very great size of their nuclei.

(3) Some of the motionless bodies have been seen to show indications of being enclosed within a delicate cyst,[1] while within others slowly revolving Ciliates have been seen.

(4) The Ciliates which emerge from these delicate hyaline cysts are frail embryos (evidently the first of their race), having languid movements, and provided only with short abortive cilia such as are indicated in Fig. 60, A and B ; while, after twenty-four hours, they assume the much more developed character to be seen in C.

(5) By no possibility could this great number of large and frail embryo Ciliates obtain an entry into the body of a fly which does not feed : they must therefore be formed from some of the tissue elements of the fly.

(6) The fact that they are only found in females that contain ova —never in male flies—together with the facts previously mentioned, as to the similarities between the immature ova and the embryo Ciliates, make the conclusion irresistible that the Ciliates are in

[1] Examination of the photograph from which Fig. 59, B, was taken with a pocket lens, shows this plainly above and to the right, almost opposite the black spot near that region.

fact formed, as they seem to be, as a result of transformations occurring in the immature ova.

(f) On the Heterogenetic Origin of Certain Ciliated Infusoria from the Eggs of a Rotifer.[1]

The weight of preconceptions against the possibility of the occurrence of Heterogenesis has hitherto been so strong as to have made it almost impossible to obtain any adequate consideration for the actual evidence adduced in favour of this or that alleged instance. But of late, preconceptions in the domain of physics and chemistry have received severe shocks, and when we are told that a so-called "element" is daily being transformed and another is actually originating from it, there appears more chance of attention being paid to the alleged existence of phenomena in the organic world which would seem to be but the carrying on into a higher platform of the familiar phenomena known as allotropism and isomerism.

Hitherto, alleged instances of heterogenesis have, without adequate consideration of evidence, been almost always assumed to be results of "infection," but it seems clear that in the cases with which the present section is concerned, any such explanation is quite impossible in regard to one of the cases, at least, in which we have masses of living matter so large that they average 1/200 of an inch in diameter, being converted, in the course of three days, into great Ciliated Infusoria of equal bulk.

We have now to consider two sets of heterogenetic transformations occurring in the great eggs or 'gemmæ' of one of the largest of the Rotifers, namely, (1) the transformation of the entire contents of a Hydatina egg into a single great Otostoma ; and (2) the segmentation of the Hydatina egg into 12 to 20 spherical masses, and their development sometimes into embryo Vorticellæ, and sometimes into embryo Oxytrichæ.

[1] This section is a reproduction, with several additions, of a paper in 'The Proceedings of the Royal Society' (B., Vol. 76, 1905). In compliance with the wishes of one of the Referees I was asked to alter the title, and in order not to run the risk of the communication being rejected I assented to a change. When sent in it bore the same title as this section, but before it was read the title became "On the Occurrence of certain Ciliated Infusoria within the Eggs of a Rotifer, considered from the Point of View of Heterogenesis." That which appears in the 'Proceedings' is little more than an abstract (illustrated) of the memoir that was sent to the Royal Society, and which is now preserved in its Archives.

(1) *The transformation of the entire contents of a Hydatina egg into a great Otostoma.*

Having witnessed, on very many occasions, the stages of this remarkable transformation of the contents of a Rotifer's egg into a Ciliated Infusorium, I am desirous of acquainting the Royal Society with the simple procedure needful to enable zoologists to study for themselves the series of changes leading to a result which many of them may be disposed to deem incredible.

All that is necessary is to procure a good stock of these large Rotifers by placing some surface mud, having a coating of Euglenæ, from a ditch in which Hydatinæ are known to exist, into a glass bowl, and to pour water very gently thereon to a depth of about 4 inches. In the course of two or three days (with a temperature of 16° or 17° C.), if the Hydatinæ are abundant, a good crop of their large eggs will be seen at the surface of the fluid, where it is in contact with the glass. The difficulty is to find suitable sites in which the Hydatinæ are really abundant. An excellent source from which I formerly obtained supplies has since been destroyed.

By the aid of a scalpel passed along their track for a short distance, groups of 20 to 30 eggs may be taken up at one time, and gently pressed off the edge of the blade into a small, white stone pot full of water. Some of such small masses of eggs (mixed, perhaps, with a few Euglenæ) will float, and others will sink. After seven or eight of these masses have been gathered and deposited, the cover should be placed upon the pot so as to cut off from the eggs all light rays, both visible and invisible.[1]

If the supply of eggs will admit of it, two other pots should be similarly charged; but if there are not enough eggs for this purpose, the two other pots should be charged on successive days with fresh batches of eggs. The larger the supplies of new-laid Hydatina eggs the more convincing will be the result. Thus on one occasion, when my supply was very abundant, gatherings of eggs were made at intervals of six hours; and among such eggs as were subsequently taken from the pots I found that *from 12 to 25 per cent. yielded Otostomata rather than Hydatinæ.*

When the pots have remained covered and undisturbed for 36 hours at a temperature of about 17° C., one of them may be opened, and some of the small masses of eggs from the bottom

[1] Full details as to the best means of obtaining supplies of Hydatinæ, of dealing with them, and subsequently obtaining the largest proportion of freshly laid eggs, will be found in my "Studies in Heterogenesis," pp. 125 and 286

of the pot should be taken up with a tiny pipette and placed in a drop of water on a microscope slip. Before covering the specimens, a minute fragment of a cover-glass should be placed at each side of the drop of water, so as to protect the delicate eggs from undue pressure.

On examination by a low power of the microscope it will be seen that there are many empty egg-cases; that within some eggs there are embryo Hydatinæ in different stages of development; while within the remaining eggs the contents will be wholly different, consisting of an aggregate of minute pellucid vesicles (each containing a few granules), together with a variable amount of granules interspersed among the vesicles.

If the cover should be again placed upon this pot, with a view to the examination of other portions of its contents 24 or 36 hours later, and this examination is made, it will be found that no further advance has taken place; that eggs which presumably were previously in the vesicular condition still remain in this stage, and are, in fact, no further developed than those of their fellows which were previously examined.

I have found on many occasions that the opening of a pot at an early stage of the transformation—even for only four or five minutes—arrests the whole process of change. It was for this reason that I advised three separate pots to be charged, so that their contents might be examined at different periods.

When, however, a second pot is opened two and a-half or three days after the eggs have been placed therein, and portions of its contents are examined in the same way, a larger proportion of empty egg-cases will be seen. There may be few or even no developing Rotifers still remaining within the eggs; and in other egg-cases, instead of the motionless vesicular contents previously seen, a great Ciliate may be found slowly revolving, or else, under the influence of the light, rupturing the egg-case, struggling out, and swimming away with rapid movements, partly of rotation. Some of the Infusoria, before they emerge, undergo segmentation into two, four, or, rarely, even into eight smaller Ciliates.

As a control experiment, it will be well, at the time that the pots are charged, to place two or three batches of the eggs with some of the same water into a watch glass, which is left exposed to light; and at the expiration of three or four days, as well as at later periods, to search among its contents for any of the same large Ciliates (which, when moving, could be easily distinguished with

a good hand lens), and also for any eggs in the intermediate vesicular stages above referred to. I have invariably found that such a search yielded only negative results.

In taking batches of eggs, in the manner indicated, to be placed in the pots, individual eggs will necessarily be of different ages. Some will have already begun to develop into Rotifers, and some of these, under the altogether unnatural conditions to which they are subjected in the dark pots, are apt to become more or less malformed as development proceeds. Others, that have been quite recently laid, will not have begun to develop, and it is these latter eggs apparently, which, under the cutting off not only of ordinary light but probably of some invisible light rays, become speedily transformed into great Ciliated Infusoria. Cutting off ordinary light rays alone from the eggs, by placing them in a small covered glass dish shut up in a cupboard or box, and maintained at the same temperature as before, seemed at first not to lead to similar results; but I subsequently ascertained that the transformation will occur under such conditions, though only after the lapse of about nine, rather than three, days. It looks, therefore, as if the stoppage of some invisible rays, capable of passing through wood but not through stone, notably hastens the transformation.

After this brief summary of the course of events during this remarkable transformation, the details may now be illustrated by a series of photomicrographs.

The changes in question occur with equal proportionate frequency in the large and the small eggs laid by Hydatinæ. It needs only a comparatively brief acquaintance with these creatures to recognise that their eggs vary a good deal in size. It is commonly believed, and I think with truth, that the smallest of the eggs give birth to the small male Hydatinæ. But between such small eggs and the large eggs there is no abrupt demarcation. There are plenty of eggs also of intermediate dimensions, and these give birth to female Hydatinæ, although to specimens falling short of the full size.

I shall show some of the stages of these transformative changes, leading to the production of Otostomas, first of all in large eggs, and subsequently in the small specimens.

In Fig. 61, all the components of which are magnified 250 diameters, the early changes leading to the production of the great Ciliate are represented. One of the large eggs recently laid is

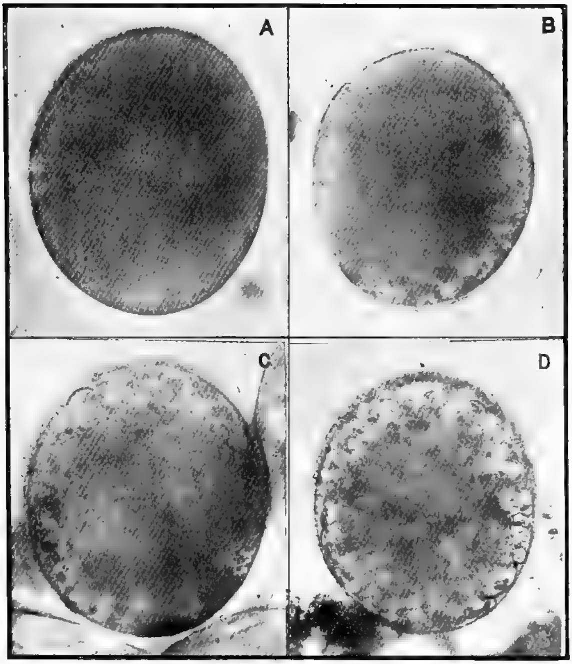


Fig. 61.

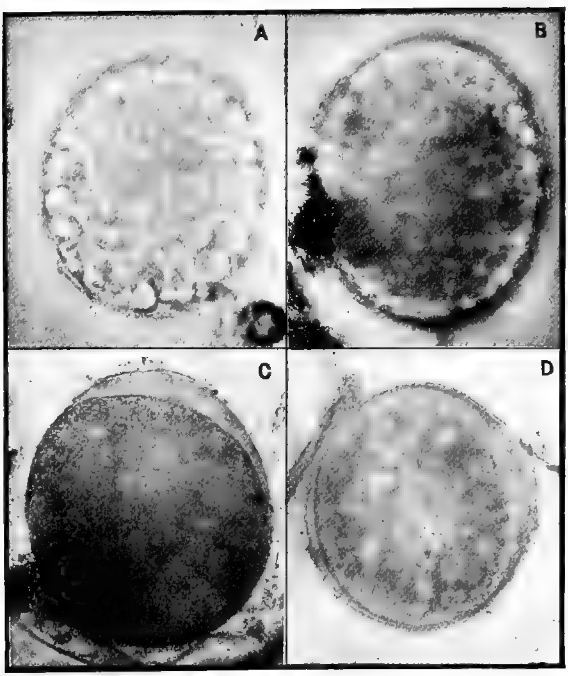


Fig. 62.

To face p. 266.

shown in A, presenting the usual evenly granular condition. In B we may see the first indications of the heterogenetic change visible at the periphery, especially below and to the right. Such changes, further advanced, are shown in C ; and further advanced still in D, where the whole egg substance is evidently becoming converted into a mass of hyaline vesicles, intermixed with granules and containing granules in their interior, *an appearance which is never seen in Hydatina eggs undergoing their normal development.* Similar changes are also to be seen in other eggs represented in Fig. 75—those in A seeming to show the gradual differentiation of the vesicles from the egg substance.

The amount of granules within and between the vesicles varies extremely in different specimens. Sometimes, as in Fig. 62, A (× 250), there are comparatively few in either situation. The egg is then pale, and the individual vesicles being very distinct may be seen to vary a good deal in size. At other times the granules in both situations are much more abundant, as in B (× 250), and it is only at the periphery of the egg that the vesicles can be distinctly seen. Occasionally, as development proceeds, the granules tend to increase again, so as sometimes almost completely to obscure the vesicular structure. Such a condition is shown in C, in which the contained body was still motionless, and in the stage just preceding the development of cilia. In D, on the contrary, cilia had just become developed, and the embryo Ciliate was beginning to revolve in a slow and almost imperceptible manner, so that a weak osmic acid solution had to be run in beneath the cover-glass before the photograph could be taken. No cilia are to be made out, because within the egg-case—the outline of which is distinctly to be seen to the left and below, as it is in C—the Ciliate is still enveloped by a closely-applied hyaline membrane.

The existence of this hyaline envelope (which, though itself mostly invisible) by its presence conceals the cilia, is well shown in Fig. 63 (× 250), representing a specimen formerly revolving, which was killed by a strong osmic acid solution, and then preserved in a mixture of glycerine and water. It will be observed that the egg-case has been broken, and that the embryo Ciliate stained by the osmic acid, has shrunk to a notable extent, together with its hyaline envelope, so that no cilia are visible.

Another of these embryos, found among a number of empty egg-cases, was seen slowly revolving within its hyaline envelope, and is shown in Fig. 64 (× 250). The specimen represented in Fig. 65

was also first seen revolving, though rather more quickly, within its egg-case. After the application of a weak solution of iodine its movements became so rapid that it burst the egg-case, though it was killed before it could completely issue therefrom. Indications of cilia are to be seen at its anterior extremity. These speedily become much more distinct; thus Fig. 66 shows a free swimming specimen, which was killed with a weak solution of iodine soon after emerging from the egg-case. It presents the common appearance of these organisms as they escape: the body being densely packed with brownish globules, which are apparently modified representatives of the original vesicles so abundantly present in the earlier stages of the transformation.[1] Sometimes, however, such globules are scarce or almost hidden, their place being taken by mere granules, and then the large elongated nucleus is occasionally visible without the aid of reagents, as in the small specimen shown in Fig. 69, A.

Whether the organisms have the body densely packed with brownish coloured globules on escaping from the egg-case, as in Fig. 66, or with mere granules as in 69, A and D, I have found to depend, in the main, upon the length of time they have been revolving within the egg-cases previous to their escape. If the pots are opened soon after their complete formation, the Ciliates are found to be densely packed with globules; but if they are not opened for two or three days after the time in which they are usually formed, the Ciliates are found still revolving within their egg-cases, but containing only granules in their interior. It would appear that, after the complete formation of the Ciliates, they go on slowly revolving within the egg-cases, so that the globules are probably, in part, used up as food products during the two or three days in which they are doing this work, while obtaining no food from without. As a rule, it is not till the pots are opened that the Ciliates, under the stimulus of light (and especially the concentrated light of the microscope lamp), begin to move more rapidly within the egg-cases, and succeed in effecting their escape. Once free, they dart away with the most rapid movements, associated often with rotations of the body on its longitudinal axis.

Occasionally, while still within the egg-case, the embryo Ciliate divides into two, and one of the products of such a division is the

[1] These more opaque globules of the developed organism often assume their original vesicular appearance, when the specimen is mounted in a mixture of glycerine and water.

FIG. 63.

FIG. 64.

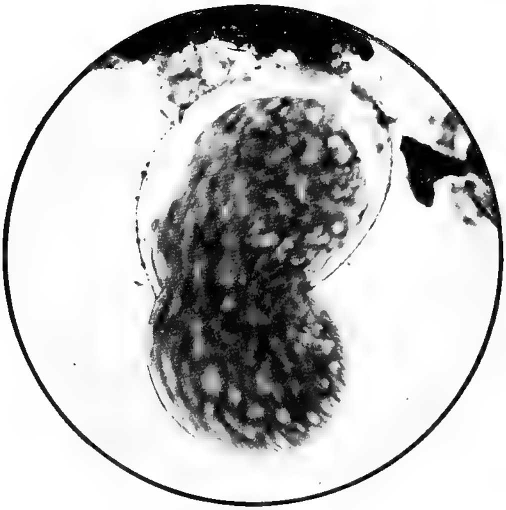


FIG. 65.

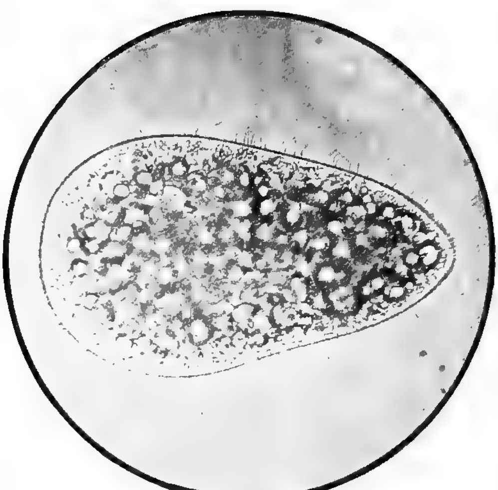


FIG. 66.

specimen shown in Fig. 69, A (× 250), in which the large elongated ovoid nucleus is partly revealed. In C (× 250) another specimen is shown, in which segmentation into four has taken place, one of the segments being at a deeper level, and therefore hidden. This specimen has been forced out from its egg-case by the pressure of the cover-glass, though the segments were still contained, and were revolving, within the invisible hyaline envelope. One of the segments into four of another organism, from a pot which was first opened on the seventh day, is shown in B, in its free swimming state. It is rather more elongated than usual and its body contained blackish granules, rather than the brownish globules more commonly to be seen. In D a free undivided specimen is shown which had probably been long revolving within its egg-case, from which it had, however, only recently escaped. It is altogether unusually free both from globules and from granules, and therefore serves well to show the fine longitudinal striations of the body along which the rows of cilia are found; and also the small characteristic ear-like mouth, the existence of which caused H. J. Carter, who first discovered the type of this genus in India, to give to the genus the name *Otostoma.* His type specimen was found within the decaying filaments of a species of Nitella. The specimens derived from the transformation of the eggs of Hydatinæ evidently belong to the same genus, as may be seen from its description, together with that of the species discovered by Carter, as given in Saville Kent's "Manual of the Infusoria" (vol. ii., p. 500, Pl. XXII.). In the specimens as they come from the Hydatina egg-case no contractile vesicle is usually to be seen, though a single complicated vesicle develops later; but in regard to the approximate size and shape of the organism, its longitudinal striations, the character of the nucleus, together with the situation and shape of the ear-like mouth, there is the closest accord between it and the form to be found at times within decaying Nitella cells.

That similar transformations to those described above occur within the small male eggs may be seen by the photographs composing Fig. 67, all of which have been taken at a magnification of 250 diameters. I only show a few of the stages, omitting other intermediate states of which I have representations, as the changes are so obviously similar to those occurring in the larger eggs. A shows a newly-laid specimen of one of these small eggs. B shows one of the early stages of the transformation in which characteristic

vesicles are beginning to form, such as are to be seen well developed in C. In D we have a representation of what was a very slowly revolving Ciliate within one of these small egg-cases, and still within its hyaline envelope, so that no cilia are visible. While in E we have a typical but small Otostoma formed from one of these small eggs, which has just escaped from its egg-case. By way of contrast a small Hydatina, just after its emergence from the egg-case, is shown in Fig. 68 (× 250). The latter is, of course, a comparatively high multicellular organism, while the heterogenetic product is a comparatively simple unicellular creature—belonging therefore to a totally different class.

My experience, since the discovery of this remarkable transformation of the substance of the Hydatina egg, has been such as to cause me no little surprise, owing to the incredulity and scepticism I have had to encounter during my efforts to make this discovery known to biologists generally, in the usual manner, through the medium of the Royal Society and one or other of the European Academies of Science. It will be well for me, therefore, to add some additional elements of proof to those already furnished.

To any one who has worked at the subject, as I have done, for some months and has seen hundreds of these great Ciliates, when taken from the pots, either in or emerging from the Hydatina egg-cases, there could be no room for doubt as to the reality of this transformation—especially when none had ever been seen under any other circumstances in association with similar material. Those, however, who have been told of these changes, even when they have seen the photographs and some of the specimens, have, in many cases, remained profoundly incredulous. Thus, one eminent Professor of Zoology, in reply to an invitation to come and see some of the living specimens, wrote as follows: "Nothing short of the demonstration by actual observation, continuous and unbroken, of the links connecting the egg (whether partially developed or not) with the Infusorian, would convince me of the accuracy of your conclusions." This was a requirement not likely to be satisfied, seeing that the change, as I then knew it, took place only when light of all kind was excluded, and was stopped as soon as light was admitted. I could only reply, therefore, that I trusted to find others who would be guided by reason, as well as by observation. Another former Professor of Zoology was so sure of his own ability to gauge the potencies of natural phenomena,

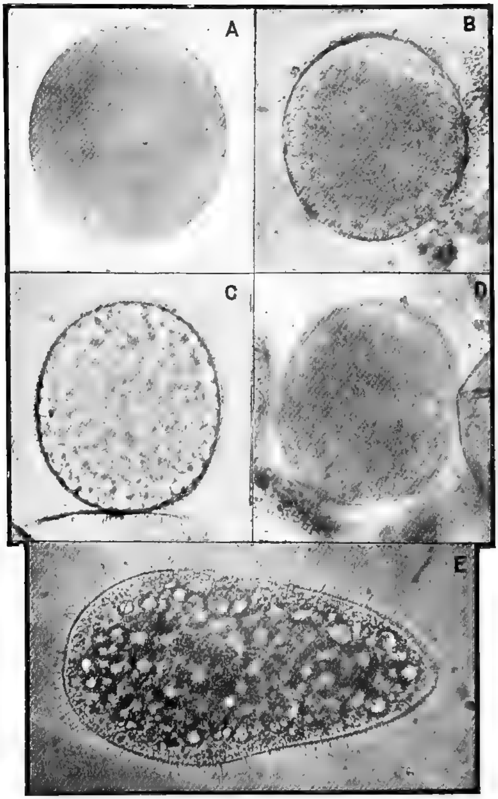


FIG. 67.

FIG. 68.

To face p. 270.

that he refused even to look at the specimens or photographs, close at hand, in illustration of this remarkable transformation.

Yet one may almost see the eggs laid by the Rotifers, busy in their midst: for the eggs are large enough to be detected with the naked eye, and can be most easily recognised with the aid of a pocket-lens. When they have accumulated sufficiently at the side of the vessel, masses of 20 to 30 or more may be taken up with a knife, and transferred to a drop of water on a microscope slip. We may then see that the little white spheres are all Hydatina eggs, either newly laid or in different stages of development, and that there are no Ciliates among them either free or encysted; we may place these masses of eggs in the pots, as I have directed, and in three or four days we may open the pots, and often find in each batch of eggs, if they had been freshly laid when collected, from 3 to 10 of the great Ciliates revolving within as many of the egg-cases. And if we open the pots earlier, we may see the intermediate stages in the transformation, as they are shown by my photographs.

It seems difficult, even allowing for a large amount of natural incredulity at first experienced, to put more than one interpretation upon such facts. The unbiassed exercise of reason upon the evidence adduced ought to make it clear.

I have, however, other proofs which, added to what I have already set forth, should suffice to bring conviction even to the most sceptical. To one of these further proofs I will now direct the reader's attention.

During the last three or four years I have often observed that when Hydatinæ have been kept indoors for ten or more days, they lay eggs very much darker in colour than usual—so that they have an almost black appearance, owing to some slight alteration in the nature of the granules entering into their formation. About the same time 'resting eggs' begin to appear, and also male Hydatinæ.

I am aware that some authorities regard it as settled that the Hydatina 'resting eggs,' are eggs which have been laid by fecundated females. Although I am not prepared to traverse this view, my experience leads me to think that 'resting eggs' are often found in abundance before male Hydatinæ make their appearance; and I am inclined to think that the formation of such eggs is intimately related to that change in the constitution of the egg to which I have just referred as occurring

almost invariably when these Rotifers have been kept long in confinement. All stages may, indeed, often be seen between the ordinary eggs full of blackish granules that are to be found in these media (both free and within the bodies of the Hydatinæ), and the similar black granule eggs to which a thick hirsute envelope is added—that may also be seen both free and within the bodies of the parent Hydatinæ.

When masses of eggs of this kind, which for brevity we may speak of as 'black granule eggs,' are taken and batches of them are placed in pots in the usual way, those that are freshly laid undergo the transformation into Otostomata, in the manner I have described; and the Ciliates to which they give rise are also filled with black granules during their formation and when they emerge from the egg-cases. Thus Fig. 70, A (× 125) shows one of these black granule eggs; while B (× 125) shows another that had gone on to the formation of a Ciliate densely packed with black granules. This was now segmented into two, and, before the application of a formalin solution, each segment was very slowly moving within the egg-case. Although no large corpuscles are to be seen in the photograph, yet examination of the specimen with the microscope showed that they were present though hidden by the abundance of granules. Of course no such complete segmentation as this, with slow rotations of the separate parts, is ever to be seen during the ordinary development of a Hydatina egg.

In D (× 250) we have represented a specimen of one of the great Otostomas, just after it had emerged from the case of one of these black granule eggs. The granules in it do not completely hide the globules, and the appearance of these within this ciliated organism is strikingly like the vesicles which enter so largely into the composition of the egg-mass during earlier periods of the transformative process. C (× 250) shows one of the products of fission of a Ciliate formed from such an egg. In it the granules are again so abundant as almost completely to hide the usual great globules which it also contains. In E (× 80) there is shown, at a much lower magnification, a group of Otostomas produced from black granule eggs, situated among a mass of egg-cases from which Hydatinæ had escaped. Most of the Ciliates were seen revolving within their egg-cases, though others had escaped and, before they were killed with a weak formalin solution, were slowly moving among the empty cases. These empty egg-cases had been left by Rotifers which had already commenced their development before having

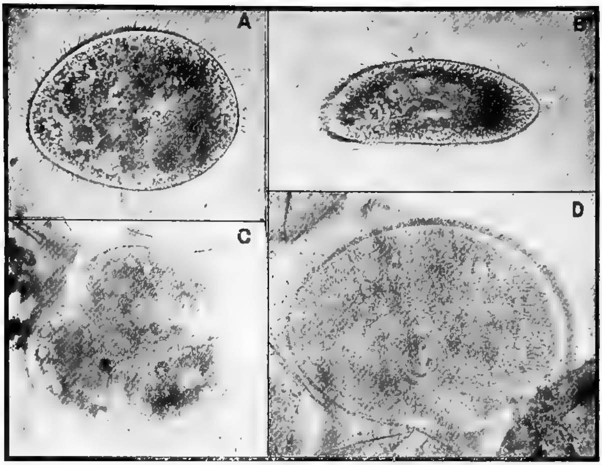


FIG. 69.

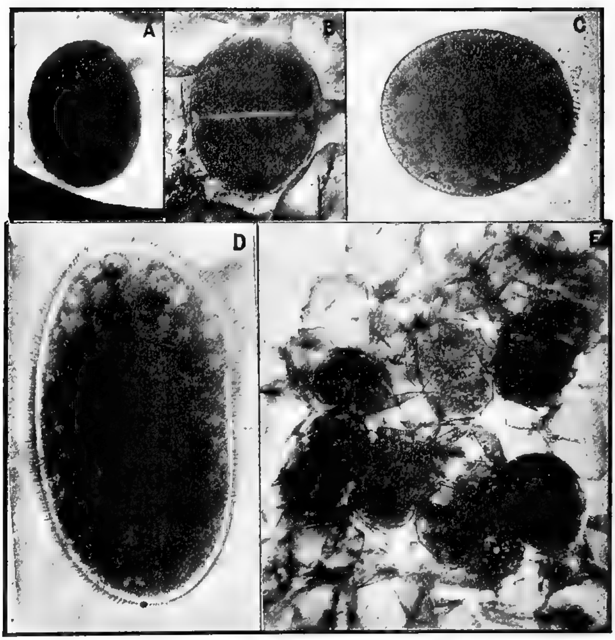


FIG. 70.

To face p. 272.

been placed in the experimental pot: and as soon as their development is completed they commonly make their way out—not waiting for the stimulus of the light as is commonly the case with the newly-developed Otostomas.

These observations are surely very conclusive. Directly the eggs vary from their usual condition, and become filled with black granules, we find a concomitant variation in the Ciliates issuing therefrom—which is, of course, only to be expected if the Ciliate is formed from the substance of the egg.

But even this does not exhaust the evidence telling in favour of the reality of this transformation of the whole substance of the newly-laid Hydatina egg, when placed under certain abnormal conditions, into a great Ciliate belonging to the genus Otostoma. There is the additional fact that I have seen eggs, in the intermediate or vesicular stage of this transformation, within the body of a dead Hydatina; and the further fact that I have seen a fully-developed active Otostoma within the dead body of another Hydatina. These observations have been made under circumstances which I will now detail.

When one of the experimental pots was opened at the end of the second day in order to obtain specimens of the eggs in the intermediate vesicular stage of the transformation, after having taken out all the batches of Hydatina eggs that were visible at the bottom of the pot, I noticed about a dozen separate bodies just the size of Hydatinæ. These were taken up with a small pipette, and on examination I found that they were really dead Hydatinæ, in different stages of decay, but that nearly all of them contained two or three eggs each, in different developmental conditions, though all were about equal in size—a condition of things never to be seen in living Hydatinæ. In one, for instance, there were the three eggs that are shown in Fig. 71, A (× 150). The upper one is a black granule egg apparently in a mature condition; another may be in an early stage of normal development; while the light egg contained a moving and nearly fully-developed Hydatina.

The contents of another of these dead Rotifers is shown in B (× 200). Here also there were three eggs; the substance of one of them had almost disappeared, but the other two undoubtedly represented different stages in the transformation of the eggs into Otostomas. In the one in which the changes were most advanced, the vesicular condition was most distinct. This is a condition

invariably met with where the contents of the eggs are being transformed into Otostomata, though nothing like it, as I have said, is to be found during the normal development of the Hydatina egg. I have found eggs in a very similar condition in two others of this group of dead Rotifers; but since then, though I have many times searched for them, I have failed to find eggs developing, either normally or abnormally, within dead Hydatinæ. This is rather remarkable, and in great contrast with what obtains among the dead Callidinas from the Luchon lichen.[1]

Some explanation of this difference may possibly be found in the different media from which these two kinds of Rotifers are taken. The Hydatinæ commonly exist in more or less foul waters, and, perhaps as a consequence, after their death they are found to rot rapidly and disappear, leaving in the course of two or three days only a heap of granular *débris*, the nature of which is indicated merely by the presence of the pharynx—which resists decay for a longer period. With the Callidinæ, however, even after the lichen has been soaking in distilled water for six or seven days, the medium is different. Although it may and does commonly contain plenty of Bacilli, the bodies of these dead Rotifers will remain in the fluid for many days without undergoing disintegration, and during this period any eggs that they may contain grow and develop. That eggs, which are small at the time of the death of the Callidinæ, grow so as to attain their full size is evidenced by the fact that though, as with Hydatinæ, it is rare to see these living Rotifers bearing more than one fully-developed egg at a time, it is quite common, as I have shown, to find within them, when dead, from two to four eggs of equal size and in different stages of development.

These facts seen to point to the conclusion that the Hydatinæ found by me in this one experimental pot may have come from some medium purer than usual, and that they must have died nearly simultaneously, from some unknown reason. They consequently did not rot so quickly, and there was time for the eggs to grow and undergo this or that stage of development. Once afterwards I found some Hydatinæ, which had been placed in a watch-glass about thirty-six hours previously, dead and containing also two or three equal-sized eggs. What had caused them to

[1] See "Studies in Heterogenesis," pp. 119-124, for an account of the transformation of the eggs of these Rotifers into a different kind of Ciliate belonging to the genus Glaucoma.

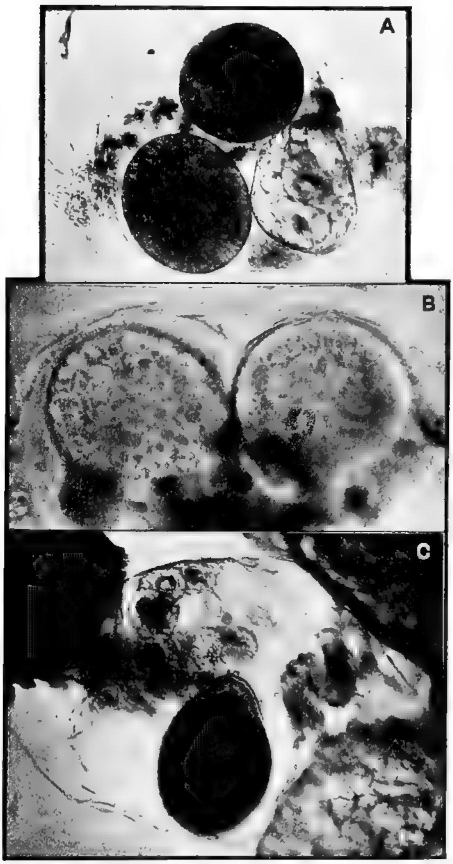


FIG. 71.

To face p. 274.

die in this way I could not discover; and in various attempts that have since been made I have never succeeded in annulling the life of the Hydatinæ without destroying the vitality of their eggs.

On another occasion a living Otostoma was found within the body of a dead Hydatina under the following circumstances. I had a stock of these Rotifers in confinement for twelve days, and the vessel in which they were contained had been standing on the mantelpiece of my study, under a bell-jar, for the greater part of that time. Much evaporation of the fluid had taken place, and no distilled water had been added to compensate for the loss. On examination I found, mixed with the Euglenæ on the surface, numerous amorphous saline concretions resulting from the evaporation; also 'resting eggs,' and many ordinary black-granule eggs, together with Hydatinæ containing eggs of each variety. I took up an isolated batch of these eggs, in association with some Euglenæ, two or three of the Rotifers, and some of the saline concretions, and placed them in a drop of distilled water under a cover-class. On examining this specimen with the microscope I found that several of the eggs seemed to be freshly laid, so I resolved to try whether a transformation of any of them could be brought about under the cover-glass—which was prevented from pressing upon the eggs by the presence of the saline concretions. I accordingly placed a shallow layer of water on the wrong side of one of a pair of 'tinting saucers,'[1] placed the microscope slip within it, and over it the other saucer right side downwards: the two together making an excellent damp chamber, with a very limited air space, in which the specimen would be cut off from light rays of all kinds.

When the specimen was looked at, after the expiration of three days, about one-third of the water beneath the cover-glass had evaporated, but on examination with the microscope I found three great Otostomas revolving within unruptured egg-cases; and, within the body of a dead Hydatina, which was only partially decayed, so that its integument was quite whole and remains of its internal organs were distinct, was a great Otostoma densely filled with black granules, free, and moving about in a very active manner. The whole body of the Rotifer, when thus seen, was visible, but unfortunately, after running a weak osmic acid solution

[1] Made of white earthenware, about four inches in diameter, and obtainable from Messrs. Swift and Son, of 81, Tottenham Court Road.

under the cover-glass, the Rotifer became partly covered by some of the saline concretions, and after various ineffectual attempts to dislodge it I was obliged to be content with photographing it as it is seen in Fig. 71, C (× 150). No other free Otostoma was found beneath this cover-glass. There were only the three within egg-cases, and the one free specimen within the body of the dead Hydatina. This was a very encouraging result, and I have several times tried to get a repetition of it, but as yet ineffectually.

In the months that these observations were being made, and previously, during prolonged work with other materials taken from the same sites, no Otostomata had ever been seen in association with Hydatinæ, except those that had been taken from the experimental vessels. On two occasions since, however, though from wholly different localities, Otostomata have been found pretty abundantly in association with Hydatinæ.[1] The adult forms have been found to be much larger, having from two to three times the length of the great embryos which issue from the egg-cases; and also to be more highly organised, seeing that what appeared to be a simple contractile vesicle has only once been seen in one of these embryo forms, while it is always present in a developed form, in association with 10 to 12 spindle-shaped radiating channels in the adults. A small specimen of one of these Otostomata, leading a free life, but in a starved state, is shown in Fig. 72 (× 250), in which the great nucleus and the small ear-shaped mouth are to be seen.

Many of these adult specimens I have been able to keep for two months, and have seen them pass into an encysted condition. When in this state their bulk is several times greater than that of Hydatina eggs. They are, likewise, enclosed in thick cyst walls, wholly unlike the thin egg-cases of the Hydatina. A rather small specimen in this condition is shown in Fig. 73 (× 250).[2]

[1] See "Studies in Heterogenesis," p. 136. It is, of course, quite possible that on certain occasions freshly-laid Hydatina eggs in ditches may be submitted to much the same kind of conditions as those to which they would be exposed in my experimental vessels. That such a transformation does not occur oftener depends probably upon the fact that it can only be brought about in eggs which have been quite freshly laid, and which then, by some chance, have been partially buried or obscured. Its rarity is indicated by the fact that an Otostoma is only recorded by Saville Kent as having been found once in this country, while this Ciliate is not once referred to by Hickson in his section on the Ciliata in Ray Lankester's "Treatise on Zoology."

[2] The subsequent fate of some of these encysted specimens is described in "Studies in Heterogenesis," p. 285.

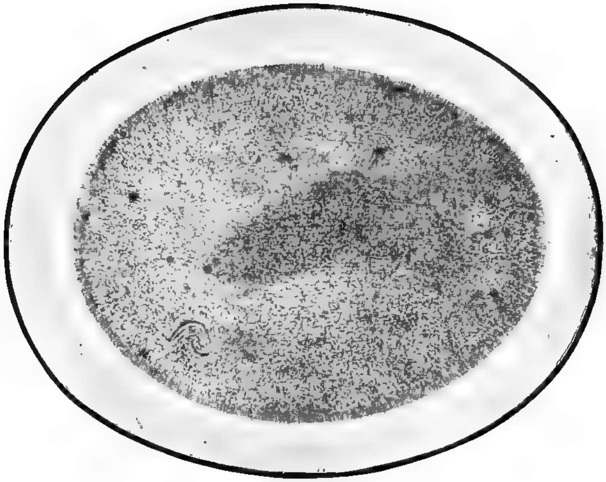


Fig. 72.

Fig. 73.

To face p. 276.

As to the reality of this transformation there is, in fact, absolutely no room for doubt. There are only two possible sources of error: (1) the mistaking encysted Otostomas for Hydatina eggs, and (2) the risk of 'infection.'

The first possibility, which was scarcely worth mentioning in connection with any competent observer, has been completely dissipated by the discovery of the great bulk and the thick cyst of the mature encysted Otostoma, together with the recognition that the product emerging from the egg-case, notwithstanding its size, is only an embryo; while the second possible source of error is shown to be equally untenable, because if a germ of the Otostoma existed which could penetrate the Hydatina egg and could devour its contents, any such young Ciliate should be visible during the process—if not at first, certainly when and after it had devoured one-half of the contents of the egg. *But no young Ciliate devouring its contents can ever be seen within the egg-case.* On the contrary, the photographs show a peculiar and definite set of changes taking place throughout the whole substance of the egg, and, as a sequel, the entire contents revolving in the form of a great embryo Ciliate.

(2) *The Origin of* 12 *to* 20 *Vorticellæ or Oxytrichæ from the substance of a single Hydatina egg.*

These are most remarkable variations, which at different times have been occasionally met with in Hydatina eggs taken from the experimental vessels. Both of these changes have only been met with in eggs found at the surface of the fluid in the experimental vessels; while the transformation of the egg-mass into an Otostoma seems to occur more abundantly, away from the air, in eggs which drop to the bottom of the pots. The changes into Oxytrichæ have been met with in the spring on three separate occasions; those into Vorticellæ only once, and that in the autumn.

The first occasion on which the transformation of the Hydatina egg-substance into Oxytrichæ was met with was in 1872, and is thus referred to in "The Beginnings of Life" (vol. ii., p. 489):—"The substance of some of the large thin-walled 'eggs' of *Hydatina senta* was seen to have undergone segmentation into about 16 spheres, each 1/1000″ in diameter. The external layers of these soon became condensed into cyst-walls, whilst the internal substance of each of them, after undergoing a series of molecular changes, resolved itself into an embryo *Oxytricha*, some of which

might be seen revolving within their cysts. Some of this batch of Rotifers' 'eggs' were seen to be filled with such spherical masses, whilst others were observed in which a few of the embryos had escaped from their cysts, and were swimming about as well-marked specimens of *Oxytricha*, within the thin investing membrane of the Rotifer egg."

Unfortunately, nothing was stated, either in my note-book or in the work itself, as to the conditions to which these eggs had been subjected, and it was my quest in this direction, resumed about four years ago, that finally led to the discovery of the origin of Otostomata and of Vorticellæ, as well as to the repetition of my observations concerning the heterogenetic origin of Oxytrichæ, from Hydatina eggs.

In the spring of 1901 I began to subject Hydatina eggs to different conditions in the hope of again bringing about this transformation into Oxytrichæ. After many unsuccessful attempts, I at last found, early in the month of May, that what seemed to be the way to obtain this change was to place portions of Euglena pellicle containing the Hydatina eggs (or else twenty or more eggs carefully scraped with a scalpel from the side of a glass vessel on which they had been laid) in some water in a small earthenware pot, in the manner already described. This final clue was obtained with a very small quantity of remaining material, and unfortunately owing to the previous setting in of dry weather, and the consequent drying up of my sources of supply of Euglenæ, I was, much to my regret, unable to carry on the investigation.[1] Still, examination of the small amount of material enclosed in this last pot revealed, on the fourth day, a ruptured Hydatina egg-case, within which were three spheres exactly similar to what I had seen in 1872, and which on measurement proved to be also about $\frac{1}{1000}''$ in diameter. One of these spheres contained a small Ciliate then actively revolving within its cyst; and in addition to the spheres there was a small free Oxytricha moving about within the egg-case. The movements of these organisms were arrested with a dilute iodine solution in order that they might be photographed; but unfortunately this led to the dissolution of the free Embryo Oxytricha, owing to its substance being so delicate, and it is

[1] At other times it was the long continuance of very wet weather which made it impossible for me to obtain supplies of Euglenæ with Hydatinæ. A moderate amount of rain, followed by some days of bright weather, is most favourable for obtaining supplies.

represented in Fig. 74, B (× 250) only by a heap of fine granules, just above the small × marked thereon. The lighter of the three spheres, which did not stain so deeply with iodine as the other two, is the one that contained the revolving embryo. Another sphere, with motionless contents, was situated in a deeper plane, and is not shown in the photograph. Close by the side of this ruptured egg there was another containing two similar spheres, and further away I found a third ruptured egg-case containing a single sphere of the same size and appearance, each of them having motionless contents.[1] I had also previously found some remarkably altered eggs which I thought might prove early stages of this kind of transformation ; and others of a similar nature have since been found in which, instead of the occurrence of a multitude of minute vesicles, all cohering together, such as occurs when the egg is being transformed into an Otostoma, the egg-substance seems to divide into, or to give origin to, a much smaller number of larger but separate units (such as are to be seen in Fig. 76), which may have the power of developing independently. In Fig. 77, A (× 250) other unequal spheres, of intermediate size, are to be seen which consequently might yield different products.[2]

The summer being unusually dry, it was not till after rain had fallen for some days that I at last succeeded, on September 22nd, in getting another supply of Euglenæ together with Hydatinæ. After placing them in water, they were left undisturbed for a few days in order to obtain a good supply of eggs. Portions of the pellicle were then transferred exactly as before to small covered pots almost full of water, and they were then kept at a temperature of about 66° F. (19° C.). Examination of a portion of the pellicle

[1] I have found that in cases where transformation of the egg-substance into an Otostoma has taken place, and the organism has subsequently undergone segmentation into four, that one of the segments has occasionally lagged behind in a motionless state, after the others have disappeared. So that, with multiple partition of the egg-mass, such as occurs when Oxytrichæ are produced, it is easy to understand that all segments may not develop exactly at the same time—hence the occurrence of remainders, such as I have just referred to, would be explicable.

[2] In connection with this point it is worth mentioning that on two occasions I have taken from one of my pots partly empty Hydatina egg-cases, each of which contained 15–20 young, active specimens of the very small Ciliate known as *Aspidisca costata*. Yet in all the other examinations that I made during the same month, and they were numerous, I only met with the specimens of Aspidisca that were within these two eggs, and a few others, at the same time, in their immediate neighbourhood.

at the end of the second day showed a large number of active and healthy specimens of Diglena,[1] and a number of young Hydatinæ whose development had progressed under these conditions, but not a single Ciliate of any kind. Examination of another portion of the pellicle at the end of the third day showed at once, to my surprise, a fairly large number of small Vorticellæ, all of about the same size. Very soon I came upon an unruptured Hydatina egg containing at least twenty spherical masses, slightly unequal in size, a photograph of which is shown in Fig. 74, C (× 250). Close by the side of this was a ruptured Hydatina egg, still containing five of the spheres (Fig. 74, D); while between the two eggs were three of the small active Vorticellæ.

My suspicion that the Vorticellæ had developed from the spheres found in the Hydatina eggs was soon confirmed by finding spheres in which the embryos were more developed and about to emerge from their delicate cysts, as in Fig. 74, E. These embryos showed contractile vesicles, and in the larger of the two there was the usual differentiation of the oral and caudal extremities. Embryo Vorticellæ (unlike the young Oxytrichæ) do not revolve within their cysts, owing to the lack of cilia distributed over their surface. Several other eggs were found in the same and in subsequent specimens from this pellicle, containing similar spheres; some of the eggs being entire, and others ruptured with part of the spheres discharged, as in Fig. 78, A. In each specimen also there were a number of the small Vorticellæ (but no other kind of Ciliate) in different stages of development, such as I succeeded in photographing in Figs. 78, B (× 125), C, D (× 250), after arresting their movements, and slightly staining them by means of a dilute solution of Westphal's mastzellen fluid. Some of the embryos were seen, just emerged from their delicate cysts, exhibiting their characteristic contractions of the posterior part of the body and the commencing formation of the pedicle, exactly as I had seen and described many years previously.[2]

Nowhere, however, could I find the early stages of this transformation of the Rotifer's egg into many Ciliate matrices. Under

[1] Specimens of Diglena have constantly been present with the Hydatinæ during these observations, and their eggs have frequently been taken from the pots, but none of them have ever been seen undergoing changes anything like those in the Hydatinæ eggs. The Diglenæ seem, indeed, quite to flourish in the dark-pots. All batches of Hydatina eggs are, moreover, not equally prone to undergo these heterogenetic changes.

[2] "The Beginnings of Life," vol. ii., p. 464.

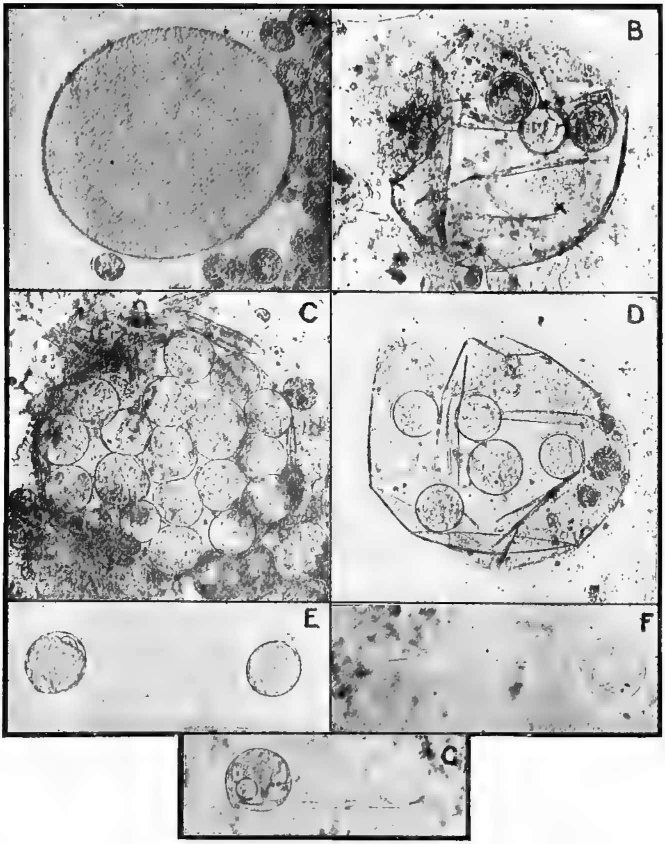


Fig. 74.

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the conditions to which these particular eggs were subjected, the complete transformation of many of them into Ciliate matrices and actual Ciliates must have been brought about within three days.

The one set of Hydatina eggs, gathered in the Spring, would appear to have yielded embryo Oxytrichæ, as they did at the same time of the year in 1872, and as I subsequently found in 1902; while the other set, gathered in the autumn, and exposed to similar conditions, yielded embryo Vorticellæ. Why this change in the transformation should have occurred it is impossible to say. One can only conjecture the existence of some very minute difference existing in the 'physiological units' of the transforming egg-mass. No difference could be detected in the early stage between the little spherical masses, but when development commenced and the embryos began to revolve within their hyaline cysts, we might feel sure that they were not to issue as Vorticellæ. The Hydatina egg produces either the one or the other of these forms—never a mixture of the two.[1]

It remained, then, more fully to study the condition of origin, and to endeavour to trace all the early stages of these remarkable transformations of the Rotifer's egg; and it was during my attempts in this direction that I discovered the equally remarkable transformation of the entire contents of a Hydatina egg into a single Otostoma, in the manner already described.

The slight gap still remains, however, in the developmental history of these cases in which a multiple partition of the contents of the Hydatina egg occurs. Yet, although the stages between those that are represented in Fig. 76, D, and the little spheres of Figs. 74 and 77 have not yet been met with, both these changes have several times been found within unbroken Hydatina egg-cases. There is little room for doubt, therefore,

[1] It is admitted that metamorphosis from one to another form occasionally occurs among the Ciliata during their stage of encystment (Carpenter on 'The Microscope,' Eighth Edition, 1901, by Dallinger, p. 780); and, among such instances of alleged transformation, it is of interest to note that the origin of Oxytrichæ from encysted Vorticellæ was described and illustrated by Pineau in the 'Annales des Sciences Nat.,' 1848 (Zool.), p. 99, and that of Oxytricha into Aspidisca by Jules Haime in the same publication in 1853 (p. 109). Although I had called attention to these transformations in 1872 in "The Beginnings of Life" (vol. ii., p. 494), and had there reproduced figures illustrating such changes, these facts had been lost sight of by me until quite recently. And yet a reference to Note 2 on p. 279 will show, strangely enough, there is some reason for suspecting that at times Aspidiscæ may also, like Vorticellæ and Oxytrichæ, be produced from Hydatina eggs.

that Fig. 74, C, represents a later stage of development of products directly traceable from the egg-substance such as are shown in Fig. 76, D. The spheres are about equally numerous in the two cases, and they are in each case also rather unequal in size. This inequality is seen in Fig. 74, D, and also in Fig. 78, A; and the uppermost of the masses in this latter figure seems to be beginning to develop into a Vorticella.

All the spheres, in fact, which during this period were produced from the Hydatina eggs developed not into Oxytrichæ but into Vorticellæ. Over and over again in the specimens examined there has been the common association of these embryo and young Vorticellæ with the unbroken or broken egg-cases of the Hydatinæ, on their removal from the dark pots. Groups of such Vorticellæ around egg-cases are represented in Fig. 78, B (× 125), and in one of these an undeveloped matrix is still to be seen. All the stages in the development of Vorticellæ from these matrices have also been traced. Young organisms, having contractile vesicles, and about to emerge from their own hyaline cysts, are shown in Fig. 74, E (× 250), while very many of them have been found with pedicles in different stages of development such as may be seen in Fig. 74, F, G and in Fig. 78, B. In many of the fully developed forms with expanded oral cilia, and very delicate transverse striæ, a slightly curved sausage-shaped nucleus has also been made out. One of these developed forms is shown with the aid of a weak solution of mastzellen stain in Fig. 78, D (× 250).

Again, in the spring of 1902 I had an opportunity of making a few more observations upon the presence of embryo Oxytrichæ and their matrices within the egg-cases of Hydatinæ. These last observations were of this nature. In a thin Euglena pellicle about nine days old, which had been kept in a very dim light, I found, at the junction of the surface of the fluid with the bowl, several small Hydatina egg-cases containing the spherical matrices as well as active young Oxytrichæ. One of these specimens, in which were contained fifteen small matrices together with one active Oxytricha, is shown in Fig. 77, C (× 200)—the Oxytricha being hidden at the time the photograph was taken. I found another egg which contained ten similar matrices with one small active Oxytricha; and also three small Hydatina eggs close together, each containing a mixture of matrices (in some of which the Ciliates were revolving), together with free and active Oxytrichæ. While arranging to

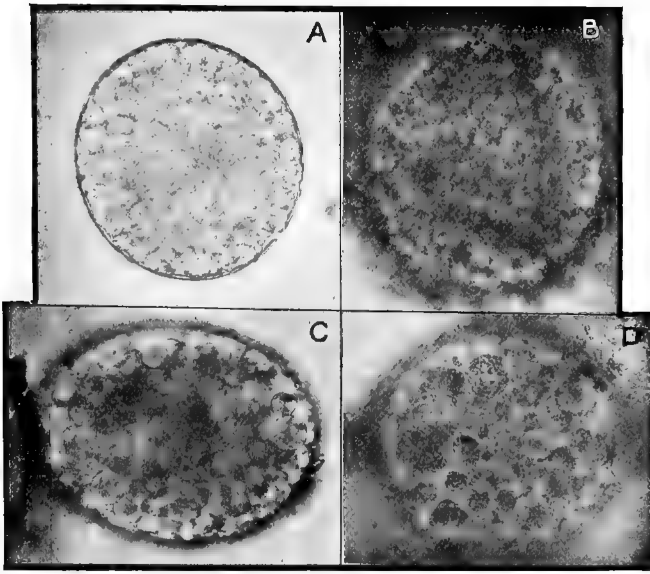

Fig. 75.

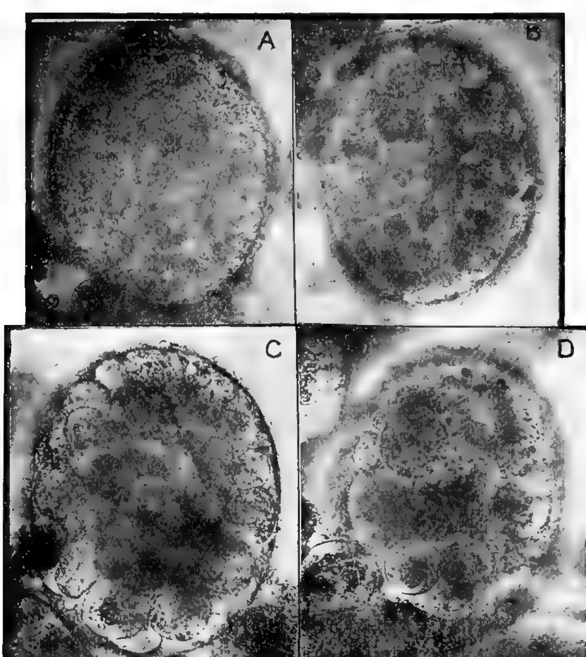

Fig. 76.

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photograph these last, I unfortunately pressed upon the cover-glass, and thus displaced and damaged the specimens. I could therefore only photograph the best of the damaged specimens, as it is to be seen in D (× 250). It contained one active Oxytricha (on the left) and four matrices, though two of the latter, as shown, are much out of focus. In one of the large eggs also, four active and larger Oxytrichæ were found, such as are shown in B (× 250), after the specimens had been altered in appearance by the weak iodine solution used to stop their movements.

Apart from the great Otostomata, only Vorticellæ or Oxytrichæ have ever been seen by me within the unruptured egg-cases of Hydatinæ, though, as I have indicated, on two other occasions I have seen as many as from 15 to 20 of the smaller Aspidiscæ partly filling ruptured egg-cases (note, p. 279). It is quite possible, therefore, that these last may also have taken origin, like the Vorticellæ and the Oxytrichæ, from the substance of the Hydatina egg. Such an egg as that shown in Fig. 77, A (several of which I have seen), whose spheres are distinctly smaller than those shown in Fig. 76, D, might yield such very small Aspidiscæ as were found in the egg-cases above referred to. These small spheres are larger than the ordinary vesicles that are produced, and which become welded together into one mass, during the production of an Otostoma. Eggs of this latter type are shown in different stages in Fig. 75, for comparison with the kind of change that leads to the formation of the little separate spheres shown in Fig. 76, D—from which embryo Vorticellæ or Oxytrichæ are presumed to take origin.

It is certainly a fact of great significance that, of all the extremely varied kinds of Ciliata, it should be precisely these three forms found by me within the Hydatina egg-cases that were demonstrated over fifty years ago, by independent observers (Pineau and J. Haime), to be convertible forms of life—to be, therefore, in a sense, analogues of those crystals whose forms and colours change under varying conditions, as a result of isomeric molecular re-arrangements, of a kind similar to those that I have been postulating as causes of heterogenetic transformations (see pp. 44–47).

There seems absolutely no room for doubt that the Vorticellæ and the Oxytrichæ *found in numbers within unruptured Hydatina egg-cases* have been produced from the very substance of the eggs by the intervention of such changes as are indicated in Figs. 76

and 74, C. This is rendered all the more certain by the fact that the Ciliates which develop from the little spheres shown in the latter figure are embryos, and that no moving thing is ever seen within the egg-cases till these little embryos become roused into active life.

The notion of 'infection' is here again an impossible one: it is absolutely incompatible with the facts. It cannot be supposed that 12 to 20 of either of these excessively delicate Ciliates in an embryo condition *could penetrate the egg-case, could devour its contents without being seen, and would then, as embryos, encyst themselves* (all in two days, or less)—*only, almost immediately after, again to pass out of their encysted condition*, and to appear as the active young Vorticellæ, or Oxytrichæ, whose development I have traced.[1]

When the facts recorded in this section are made known to other workers in different parts of the world, some of whom may have no difficulty in finding plenty of material with which to work, and who may be skilled, as I am not, in modern methods of making sections of such minute objects as Hydatina eggs, much highly interesting information, doubtless, will be forthcoming of a nature to satisfy cytologists as to the histological details of the transformation in question. These minute details, however, I must leave to others; all I claim is to have established the fact itself of the heterogenetic origin of different kinds of Ciliated Infusoria from the eggs of one and the same Rotifer. The seeming utter improbability of such a fact may be taken as some measure of its enormous importance for biological science, when

[1] It is, of course, well known that when a Ciliate which has been feeding and leading a free life encysts itself, it usually remains in this condition for weeks or even months, and forms round itself a comparatively thick cyst (as in Fig. 73), which persists after the Ciliate emerges therefrom, as seen in Fig. 40, B. But a totally different set of things obtains in regard to the embryo Otostomata, the Vorticellæ, and the Oxytrichæ, with which we are now concerned. They are each of them first seen in the Hydatina egg-cases, motionless, within hyaline cysts which they speedily leave; the cysts being so delicate that when ruptured they seem not to leave a trace behind. This is the case even with the large Otostomata. Thus, such a hyaline cyst, though so delicate as to be invisible in the photograph, was holding the segments together which are shown in Fig. 69, C; and a similar very delicate hyaline endocyst formerly enveloped the Otostoma represented in Fig. 65 as escaping from the egg-case—though no trace of it is now to be seen. Yet this delicate endocyst is plainly visible around the stained and contracted embryo shown in Fig. 63.

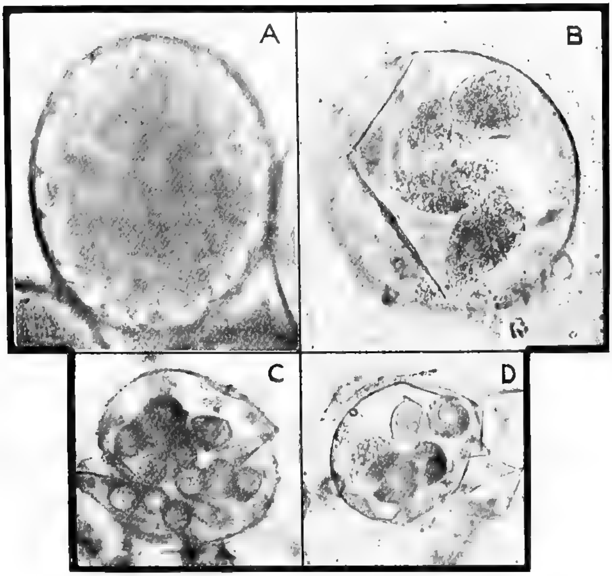

Fig. 77.

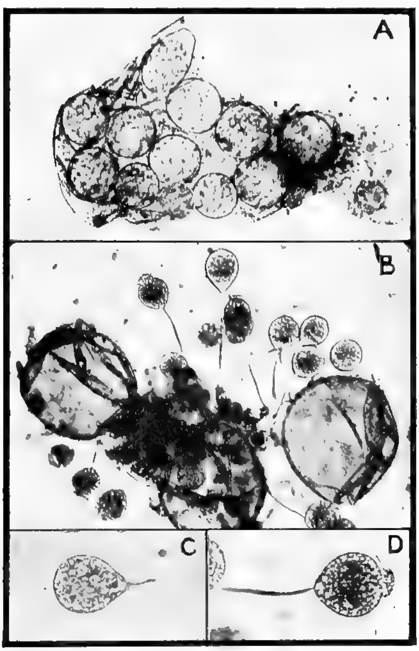

Fig. 78.

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we consider the far from simple structure of these unicellular Ciliates ; that no kinship of any sort, in the ordinary sense of the word, exists between them and Rotifers ; and, further, that even totally different forms of Ciliated Infusoria are capable of being produced from the egg-substance of a Hydatina according as it becomes transformed as a whole, or only after having undergone segmentation into a varying number of small spherical masses.

CHAPTER XIV

CONCLUSION: THE CONGRUITY OF THE EVIDENCE

IN not-living matter we have combinations of molecules of all degrees of complexity—beginning with those entering into the composition of the simple elements, and ending with the big and highly complex components of various colloids. And seeing that the so-called elements are built up by molecules composed of different numbers of similar atoms, the difference between elements and compounds simply depends upon the likeness or unlikeness of the atoms entering into the composition of their molecules.

Thus molecular composition is an important item even with reference to substances that are looked upon as elementary—different modes of composition or arrangement of the atoms of like kind sufficing to produce what are known as 'allotropic' states. It will be easily understood, therefore, that in compound substances a greater and greater possibility of molecular rearrangement arises in proportion to their atomic complexity. Gradually, in fact, this atomic complexity becomes the all-important character of a compound, and one to which the nature of the constituent atoms is altogether subordinate. In proof of this, one has only to refer to the multitudes of 'isomeric' compounds having wholly different properties, made up of carbon, hydrogen, and oxygen in the same relative proportions, and to the enormous number of other isomeric compounds resulting from the varying modes of arrangement of a few definite elements.

There can be no reasonable doubt that the form of a crystal is a resultant necessity, predetermined by the molecular properties of the matter composing it, and the sum total of conditions acting thereupon at the time of collocation.

Similarly, there are excellent reasons for believing that the form and structure possessed by each simple organism is that which

is necessitated, in the medium in which it exists, by the nature and properties of the complex organic molecules of which it is composed.

The fundamental difference between a crystal and an organism lies, indeed, in the fact that the one is a 'statical' and the other a 'dynamical' aggregate; this difference being dependent upon the great complexity of the molecules of which the latter is composed. Organisms are dynamical aggregates because among their molecules new motions and new arrangements are continually being assumed; in the course of which, in lower organisms, there frequently arises a spontaneous division of the parent mass—that is to say, 'fission' or 'gemmation' takes place.

The intimate nature of the process of reproduction characterising living things is revealed by a consideration of these processes of 'fission' and 'gemmation.' All parts of very low organisms, when thus separated from the parent, have the power of developing into living things of a similar kind. This property has points of resemblance to the process whereby a fragment *broken* from a pre-existing crystal, and thrown into a suitable solution, gradually grows into a perfect crystal, similar to that from which it has been derived. Though the fragment will reproduce a crystal similar to that from which it was taken, the crystal can undergo no spontaneous division.

If it is true, as we affirm, that Torulæ, and even Bacteria (in aggregates if not singly) are able to develop into simple forms of Fungi, and that such Bacteria and Torulæ are merely some of the primary forms most frequently assumed by certain kinds of new-born living matter, then obviously the form and structure of the Fungus would stand in the same relation to the matter of which it is composed that the form and molecular structure of the crystal does to its matter. There would be, in fact, just as much reason why the new-born organism should develop into the form of one already in existence, as there would be that the crystal of sulphate of soda which forms to-day in a solution of that substance, should resemble that which formed under similar conditions twelve months or a hundred years previously. He who believes in the uniformity of natural phenomena could anticipate no other result. Living matter, which we believe to be now produced *de novo*, speedily shapes itself into some well-known form; and so also new crystalline matter, which may have been produced synthetically by the chemist

in his laboratory, falls habitually into one or other of the known crystalline systems.

It seems, therefore, no more wonderful that the simple Mould that develops *de novo* to-day should resemble another which develops from the 'spore' of a pre-existing organism, than that a crystal forming independently to-day in a saline solution should resemble another which is capable of arising by the growth of a fragment detached from a similar pre-existing crystal. In all these cases, there is a similarity of product, because the crystalline or organic form produced is to be regarded as the physical expression of the harmonious actions which have led to its production—because the forms are the results of a physical necessity, and not of a mere blind chance.[1]

What takes place in the reproductive processes occurring among higher forms of life seems to be only a more complex exemplification of similar phenomena. As Herbert Spencer said,[2] "The assumption to which we seem driven by the *ensemble* of the evidence is, that sperm-cells and germ-cells are essentially nothing more than vehicles in which are contained small groups of the physiological units in a fit state for obeying their proclivity towards the structural arrangement of the species they belong to. . . . Thus, the phenomena of Heredity are seen to assimilate with other phenomena. . . . We must conclude that the likeness of any organism to either parent is conveyed by the special tendencies of the physiological units derived from that parent."

Looked at in this general sense the phenomena of heredity seem to be only special manifestations of the property we have spoken of as 'organic polarity'; as a result of which, as we have seen, parts of lower organisms are capable under suitable conditions of reproducing similar entire organisms, just as a part of a crystal immersed in its mother liquid will, when the conditions are suitable, reproduce the form of the parent crystal.

Neither Herbert Spencer nor Weismann alludes to the existence of transformations in organisms at all comparable with the allotropic and isomeric transformations that are so remarkable in simpler forms of matter, and which, as is well known, increase so much in frequency as the constitution of the matter becomes

[1] Yet Prof. Huxley once made some very sensational remarks to a large audience in opposition to such views ("Quart. Jrnl. of Micros. Science," Oct., 1870, p. 366).

[2] "Principles of Biology," I, p. 317.

more complex. In referring to that most complex form of matter, known as protoplasm, any analogous isomeric transformations are no longer referred to even as possible. The matter itself is admitted to be of the most changeable kind, and the forms of living things are admitted to depend upon the intimate constitution of the units of living matter from which they arise (the 'physiological units' of Spencer, or the 'ids' of Weismann), and yet not one word is said as to the existence among simple organisms of transformations of form akin to those which we know as occurring in such simple substances as carbon, phosphorus, salts of mercury and other metals, as well as in the carbon compounds, as a result of isomeric changes.

The writer, however, believes that analogous transformations of living matter are constantly occurring among lower forms of animal and vegetal life, and that these are, in part, represented by multitudinous heterogenetic transformations, some few of which have been studied in this work.

These lower forms of life, for the most part, multiply, as I have said, by mere 'discontinuous growth,' and their forms, like those of crystals, seem absolutely dependent upon their molecular constitution and the environing conditions in the midst of which they exist. Changes of this or that kind in their environing conditions, will, in many of them, lead to remarkable changes, owing to alterations thereby induced in their molecular constitution—alterations which have as their result the establishment of new formative tendencies. Moreover, I believe that many of the discrepant observations made by good observers as to the ordinary phases in the life-history of such pleomorphic species as Chlamydococcus, Pleurococcus, and many others, have been due to modifications occurring from time to time in their developmental phases, under the influence of special conditions of one or another kind, though either of such phases may ultimately revert to the form from which it started. The accounts that have been given by good observers show that a regular order is not observed, and that strange phases are, from time to time, apt to be intercalated. Many of these lower forms of life are so much the creatures of circumstances—so transitory in their duration—that I long ago proposed to speak of them as 'Ephemeromorphs,'[1] in contradistinction to the regularly recurring forms comprised under, and understood by, the term 'species.'

[1] "The Beginnings of Life," 1872, vol. ii., p. 559.

The close inter-relations and interchangeability that I have shown to exist between the varied lower types of life with which we have been concerned actually force upon us the belief, that the forms assumed are as much the immediate results of their molecular composition and their environing forces as are the various forms assumed by crystals. We are driven to the belief, in fact, that 'organic polarity' is the dominating influence in the production of this or that form belonging to the great assemblage of 'Ephemeromorphs'; that heredity in any other sense is not operative among them; and that this whole world of lowest vegetal and animal forms of life must be wholly removed from the influence of Natural Selection.

I am well aware that these are very unorthodox views—that Darwin, Weismann, Poulton and others actually believe that the rate of change is lower among lowest than among higher organisms. This most surprising view is based, in the main, upon the fact dwelt upon by Huxley and others as to the "persistence of types" through geologic ages—a fact which, with the proof of the occurrence of heterogenesis, is, as we shall see, capable of receiving a totally different interpretation, and one in no way inconsistent with the recognition of all that has been shown by multitudes of workers as to the extreme variability of these Ephemeromorphs, and the pleomorphism of such organisms as Protococcus, Hæmatococcus, Chlamydomonas, Sphæroplia and others. The changes among these latter organisms are indeed so fitful, and vary so much in their order at different times and seasons (as witness different observers) that many of their varying changes could scarcely be considered to be results of heredity. Like actual heterogenetic transformations, such irregular variations must be considered to depend upon slight isomeric changes in the molecular constitution of their 'physiological units,' acting in conjunction with organic polarity as a formative principle.

The extreme variability of the organisms referred to above is well illustrated by the researches of Prof. F. Cohn on *Protococcus pluvialis;* and such observations by an investigator of great repute may be cited as an example of many others.[1] He discovered, as derivatives of this apparently simple Alga, the most widely differing

[1] See Transln. of his memoir, with Plate, in "Botanical and Physiological Memoirs," Ray Soc. 1853. The reader may also consult Cook's "British Fresh Water Algæ," for details as to the great variability of the other forms mentioned above.

forms of Phytozoa, which were actually seen by him to be derived from one another by direct processes of transformation. In summing up the results of his investigations he said[1]: "Thus we see that a single species, owing to its numerous modes of propagation, can pass through a number of very various forms of development, which have been either erroneously arranged as distinct genera, or at least as remaining stationary in those genera, although, in fact, only transitionary stages." And, after a brief enumeration of the principal changes he has witnessed among the derivatives of this simple Alga, he adds the following important statement: "A critical and comparative consideration of the foregoing facts would therefore appear to render untenable almost all the principles which modern systematists have hitherto adopted as the basis for the construction of their Natural Kingdoms, Families, Genera, and Species."

It seems clear, in fact, that so long as organisms multiply by mere 'discontinuous growth' (fission or gemmation) there is absolutely no reason to appeal to heredity except in the form of 'organic polarity.' This would be all-sufficient for the preservation of their likeness under similar conditions; for accounting for their changes under dissimilar conditions; and for their reversion (as with crystals and double salts)[2] to old forms on renewal of old conditions—a class of facts among organisms with which the researches of many bacteriologists have made us perfectly familiar.

It is, then, not till we get out of the great Ephemeromorphic plexus, and come to organic forms which habitually multiply by sperm-cells and germ-cells (or occasionally by the latter alone) that heredity, as postulated by Darwin and Weismann, comes into play: and it is, consequently, not till we reach such forms that it would be possible for Natural Selection to be influential as a cause of evolution. I make bold to say this notwithstanding the opposite views held by Darwin himself, Weismann and others who, as is well known, maintain that the influence of Natural Selection as a producer of fitness between organisms and their circumstances is one which is applicable to all forms of life. I maintain, however, that for the chance-products originating by heterogenesis appeal must be made to 'organic polarity' alone.

The evidence for the conjoint association of Organic Polarity and Heterogenesis is far more certain, and indeed belongs to

[1] *Loc. cit.*, p. 559. [2] See pp. 44–46.

wholly different order of certitude from that upon which the co-operative alliance between Heredity and Natural Selection depends. On the one hand we have the facts directly observed and recorded in Chapters IX–XIII; while on the other Weismann himself says,[1] "It is true that we have never directly observed the process of natural selection," and he then goes on to illustrate the kind of evidence that exists for it, and its cogency, by adding, "neither has anybody directly observed the mode in which the heat of the animal body is generated by the processes of combustion going on in the blood and in the tissues, nevertheless this is believed as a certainty." We, see, therefore, that for the establishment of the doctrine of Animal Heat as well as that of Natural Selection, although the amount of direct observation has been altogether inadequate to bring about conviction, the lacunæ—as in scientific investigations generally—have been filled by the exercise of reason. Yet the tendency with my critics has been to demand nothing but observation, "continuous and unbroken"[2] and to allow no place whatever for reason, even to supply the smallest links in the chain of evidence. That this is no exaggerated statement any one may see for himself who will carefully peruse Appendix II of my "Studies in Heterogenesis," and will contrast the very cogent evidence which I adduced as to the reality of the transformation of Hydatina eggs into great Otostomata, with the incredulity with which this evidence was received, not only in this country but abroad.

So far, indeed, from there being any *à priori* objections to *per saltum* development or Heterogenesis, which, as I claim to have shown, occurs so frequently, the fact of its occurrence ought to be considered thoroughly harmonious with all that we know concerning simpler forms of matter, and with the constitution of living matter itself.

As Herbert Spencer said,[3] it is a cardinal fact "that proteids admit of multitudinous transformations; and it seems not improbable that, in protoplasm, various isomeric proteids are mingled. If so, we must conclude that protoplasm admits of almost infinite variations in nature." But the 'ids' of Weismann as well as the 'physiological units' of Herbert Spencer are essentially protoplasmic in nature and, therefore, of great molecular complexity. These are

[1] "Studies in the Theory of Descent," Trans., p. 643.
[2] "Studies in Heterogenesis," p. 130.
[3] "Principles of Biology," vol. i., 1898, p. 67.

the ultimate units, the "unknown somethings which have the power of organising themselves into a structure of this or that species."[1] I merely contend, therefore, that these ultimate units, looking to their nature and origin, must be supposed to be capable of undergoing isomeric changes within certain limits, and that, such changes having been brought about, these altered units would tend to unfold into new forms—more or less different from the parent forms, and corresponding with this or that kind of heterogenetic transformation. The number of such transformations now known is considerable among lower organisms, though comparatively rare among higher plants and animals—where they are represented by sports and the 'Mutations' of de Vries.

Great differences exist with respect to the degree of variation that may be induced in different lower organisms, within similar periods, under the influence of any given changes in their environments. Changes, which may be almost inoperative in producing a modification of some organisms, may produce profound alterations in others. And, similarly, while a very prolonged continuance of altered conditions is needful to affect some organisms, the influence of changed conditions, on others, may be rapid and more or less immediate.

The greater the differentiation and complexity of any organism, the less is it likely to be influenced by slight or temporary modifications in the 'conditions' or influences to which it is subjected. The complexity has been gradually attained, and each part or organ has functions and structures which are definitely related to the functions and structures of other parts. The whole is composed of parts working in accord with one another, and in such a manner as to establish a harmony between the actions going on within and those without the organism. The result of this interaction during past time has been the gradual elaboration of an organism of a certain structure; so that this structure and form are only to be regarded as the physical expressions of an approximate moving equilibrium between numerous related factors—between the inherent tendencies of 'physiological units' under the influence of all past 'conditions,' and the present operation of external forces upon the now-acquired structure. When, therefore, unaccustomed conditions act upon such organisms, they are unable easily, or within short periods, to produce direct modifications of

[1] *Loc. cit.*, p. 370.

the organs principally affected; because a change in one important organ would necessitate other changes throughout the whole organism, in order to establish a new balance of functions. Thus, variations which may be induced for a time in higher organisms, continually tend, when the modifying influences have disappeared, to be dwarfed and perhaps ultimately abolished, owing to the sum total of internal forces acting in such a manner as gradually to reproduce the former condition of equilibrium.

But how different are the phenomena when we turn our attention to lower organisms.

If the facts made known, and the views that I have been advocating, seem difficult of acceptance to many, as to a reviewer in "Nature"[1] who said, in reference to some of the facts, that they were "entirely beyond credence, because so meaningless," I would say, let them keep steadily in mind all that has been adduced, as matter of common knowledge, concerning the isomeric states of various saline substances, their transformations in crystalline form, in colour, and in other characters—under variations in external conditions; as well as the total transformations of such elementary substances as carbon, phosphorus and sulphur owing to alterations in the exact collocation of the atoms in their component molecules. Such phenomena may help to supply something as to the meaning of heterogenetic transformations.

These alleged facts concerning heterogenesis may also be said to be inconceivable. Granted: but that is no reason for doubting their validity. How far are the processes of allotropism and isomerism themselves conceivable, in any true sense of the word? Our notions of conceivability in regard to physical phenomena have received many severe shocks even during the last two years—by revelations concerning wireless telegraphy, the "mystery of radium," and the constitution of matter itself—that is, of the "simple atoms" of which elementary substances are composed. Thus, Sir Oliver Lodge tells us that we are now arriving at an electrical theory of matter[2]; that in an atom of hydrogen there are nearly 1,000 electrons, and in the mercury atom 100,000 electrons. But the electrons are so minute that even with these vast numbers in a single atom, they do not "fill all the space, and if the distance between them were calculated, they seemed to be about as far

[1] Feb. 25, 1904, p. 386.
[2] See "Nature," March 12, 1903.

apart in proportion to their size as the planets in the solar system."[1] While Professor Rutherford[2] has shown that the behaviour of radium, as well as of thorium and uranium, is only to be explained on the idea that they are elements in the process of slow "spontaneous transformation." Yet, during this process of molecular disintegration, radium, like its allies, "*may be looked upon as continuously giving rise to new elements by a process of material evolution.*" One of these new elements has now been definitely ascertained by Sir William Ramsay to be the well-known gas helium—first discovered by the spectroscope as one of the constituents of the sun. The 'emanation' given off from radium has all the characters of a gas, and, when kept in a tube, it changed itself first into this rare gas helium, and then into particles of positive and negative electricity (electrons). Doubtless, other of the products of transformations of this kind will soon be identified, and it will be generally recognised that even the "spontaneous generation"—hitherto unknown—of so-called chemical elements is, under suitable conditions, ever taking place around us, not only in the bodies above-mentioned but in many others according to Gustave le Bon, who looks upon electrons as intermediate stages between what we know as matter and the ether, from which, in ways altogether unknown, the different so-called elements have been derived.[3]

After such revelations, who would dogmatise and attempt to set bounds to the potencies of matter, living or not living? Let us think of what is known as to the great complexity of the molecules of proteid substances, and the further extreme complexity of the units of living matter which they help to form. Let us then attempt to add to this conception a further conception based upon these modern revelations as to the constitution of even the simplest atom entering into the formation of protoplasm, and, even if willing to accept what is attested by observation and

[1] It is supposed now by many authorities that there is "in the core of the atom nothing but a group of positive electrons, forming a body like our sun, round which their negative partners revolve at distances and in orbits corresponding not imperfectly to those of the planets . . and the difference of chemical and physical behaviour displayed by, for instance, an atom of hydrogen and another of iron, is accounted for by supposing the planets of one to be either more numerous or to have different orbits from those of others" ("Athenæum," May 27, 1905, p. 551).

[2] "The Times Literary Supplement," June 26, 1903.

[3] "L'Évolution de la Matière," Paris, 1905. He supports the view that electrons are "centres of vortical strain in the ether."

experiment as to the heterogenetic transformations of living matter, we may be perfectly free to admit our utter inability to explain the mutations in question. In this sense they may also be said to be "meaningless."

Let us now briefly enumerate some of the heterogenetic transformations which even a single observer has been able to recognise in a few years, as some indication of what might be discovered by other workers if they would only be content to search for Nature's secrets in her own laboratories, as I have done. And if, in many cases, they did not witness similar changes to those which have been here described, they would doubtless discover many other transformations just as marvellous—possibly more so.

In the foregoing pages I have shown that Bacteria of different kinds, Torulæ and Moulds, may, and undoubtedly do, arise *de novo* by heterogenesis; that various simple Algæ, Diatoms, and Phytozoa may take their origin from alien sources; and that a similar heterogenetic origin is most frequently met with for Amœbæ, Actinophrys, Flagellate Monads, Peranemata, and even Ciliated Infusoria.

The heterogenetic origin of these organisms takes place after one or other of the four following methods:—

(1) They may arise, as with Bacteria, Torulæ and some Amœbæ, by the gradual growth of previously invisible, microscopic particles revealing themselves in the substance of their matrix.

(2) They may arise from the fusion and subsequent individualisation of groups of Bacteria, as in the origin of Flagellate Monads, Amœbæ, Fungus-germs, and Ciliated Infusoria from transformed Zooglœa aggregates, in the pellicle on a hay, or other organic, infusion.

(3) They may take origin, as with other Amœbæ, Flagellate Monads, Actinophrys, Peranemata, Fungus-germs, and Ciliated Infusoria, by a simultaneous segmentation of the entire substance of some matrix into a number of more or less equal parts, each of which develops into similar representatives of one or other of these new forms of life.

(4) Or, the whole of a given matrix may be transformed into a new form of life, as when Chlorophyll Corpuscles are converted into Amœbæ, Actinophrys, or some of the simpler Algæ; when the cells of a parasitic Alga are converted into Diatoms; or when

the entire egg of a Rotifer or of a Tardigrade becomes transformed into one of the larger forms of Ciliated Infusoria.[1]

I have shown, moreover, how convertible many of these alien derivatives are—how in the pellicle on a hay infusion masses of Zooglœa are formed which segment now into Fungus-germs, now into Flagellate Monads, and now into Amœbæ ; and similarly, that the substance of a Rotifer's egg may under different conditions segment into Flagellate Monads, Amœbæ, Peranemata, or even into Ciliated Infusoria ; that, on different occasions, one or other of the same kinds of organisms may take origin from the substance of large encysted Amœbæ, while an encysted Ciliate may also, itself, break up into segments which develop sometimes into Flagellate Monads, and at others into Amœbæ, Peranemata or Fungus-germs.

I have further shown, here or elsewhere, that Confervoid cells, 'resting spores' of Spirogyra, and 'resting spores' of Vaucheria may also yield Amœbæ, Actinophrys, or Flagellate Monads; and I have demonstrated how abundantly similar organisms are born within the closed cells of Nitella, and Spirogyra, and the filaments of Vaucheria. Again, we have found all these organisms—Fungi, Monads, Amœbæ, Actinophrys and Ciliated Infusoria proceeding from the substance of Euglenæ ; while, at other times, various Algæ are seen to be derived either from their transformation as a whole or from that of their Chlorophyll Corpuscles.

However astounding these statements may seem to those who have not worked long and earnestly at such subjects for themselves, I venture to submit that the facts adduced in this volume, backed by the appearances preserved in the photomicrographs, should go far to convince those who will give the subject a fair and impartial consideration. The appearances thus recorded and preserved can neither be explained by the facile solution that they are "results of infection," nor can it be said that the new forms of life which seem to arise by heterogenesis are in reality normal and habitual phases in the life-history of the organisms from which they proceed. These other possible interpretations have always been carefully kept in view in each case, and the evidence for and against them duly weighed.

All the observations recorded in proof of the heterogenetic origin

[1] The evidence in support of these statements, and others in subsequent paragraphs, may be easily found by consulting the Index here, as well as that of my "Studies in Heterogenesis."

of Bacteria and their allies have been conducted under aseptic conditions, and with all necessary precautions against the possibility of infection. While in regard to many other observations that have been made—touching the heterogenetic origin of Amœbæ, Monads, Peranemata and Ciliated Infusoria—it must be seen that these, if made at all, have to be made on organisms either in their own or in experimentally varied environments; that, in either case, aseptic conditions, or absolute isolation of the organisms under observation, is always an impossibility; and that any attempts in this direction would inevitably stop the changes hitherto in progress.

In regard to the objection that there has not been a continuous watching of the alleged heterogenetic changes, from start to finish, on the same individual organism, I can only say that compliance with such demands would not only be fruitless but would go far to render for ever impossible any knowledge of heterogenesis. Moreover, the observation of different stages of change in different individuals is, after all, the mode by which, as is well known, multitudes of embryological investigations have to be conducted. The methods employed by those who would gain a knowledge of heterogenesis, cannot, from the very nature of the subject, be strict laboratory methods; but they are, none the less, methods similar to those by which much other scientific knowledge has gradually been built up. Those who work at this subject have to adopt methods which, though carrying with them a greater element of certainty and a larger amount of actual observation of the processes in question, are fairly comparable with those employed by geologists—we each of us strive to put the best and most reasonable interpretation upon the facts that come under our observation, as much as possible, irrespectively of preconceptions and *à priori* views.[1]

In consequence of the sceptical attitude on the part of those whose preconceptions hinder their working at such subjects for themselves, it will be well for us now to look at the questions at issue from a more general point of view. We find ourselves face to face with two antagonistic doctrines: (1) the view that living things no longer arise *de novo*, and (2) the view that living things are still coming into being *de novo* both by Archebiosis and by

[1] Those who are sceptical concerning Heterogenesis and have any desire to be convinced may easily, by following my instructions, study the changes occurring in Zooglœa masses in the pellicle on a hay infusion and the appearance of Monads, Amœbæ and Fungus-germs therein; or they may study the origin of

Heterogenesis. Let us see, therefore, which view is most in accordance with certain well-known and generally admitted classes of facts, and which view is more or less completely opposed thereto, for as Herbert Spencer has said, "There is no mode of establishing the validity of any belief except that of showing its entire congruity with all other beliefs."

The first of these classes of facts, which may be regarded as a kind of touchstone for testing the relative validity of the two views, is the *present day existence of vast multitudes of lowest organisms of all kinds*. If all the forms of life that have ever existed upon the surface of the earth have been derived from the primordial forms which took origin by natural synthetic processes occurring only in an incalculably remote past, no adequate and consistent explanation would be forthcoming of the undoubted existence, at the present day, of the teeming multitudes of such lower organisms as have been above referred to. For if the assumed gradual development of higher forms of life, during all past geologic ages, has been largely due to the intrinsic mutability of living matter, as the Evolution hypothesis assumes, would it not be a stultification of that hypothesis to suppose that such primordial forms as Bacteria, Torulæ, Monads, Amœbæ, and Ciliated Infusoria have remained practically unchanged, and in these low grades, for untold millions of years? Yet those whose views are at present most widely accepted would have us believe that ages and ages before the advent of Man or his immediate predecessors upon the earth, the ancestry of the Bacterium, the Amœba, the Monad, the Mould, or of any of the low infusorial animalcules now to be seen in their respective habitats, had been tenants of our globe. The mere suggestion seems to carry absurdity on its face. If this were really so, then we could only expect that such forms would be the very types of conservatism and stability; whereas, as a matter of fact, all

Diatoms from cells of Chlorochytrium in dead Duckweed; the origin of pigment Amœbæ in Vaucheria filaments or 'resting spores,' or some of the remarkable changes which I have described as occurring within the cells of Nitella. Some of these transformations can generally be induced or found almost at will. Others, as I have shown, are much more fitful in their occurrence, or can only be met with under conditions not always easy to realise, as with the transformation of Hydatina eggs into Ciliated Infusoria, and many other of the changes which I have recorded. This latter difficulty may be due sometimes to dearth of the proper organisms; sometimes to unknown changes in the intimate nature of the organisms not favourable to transformations previously seen—just as de Vries has found in regard to the occurrence of 'mutations,' at different times or in different species.

such organisms are rather the best types of change and mutability. That some Bacteria, Amœbæ, Moulds, and other low organisms should have lived in unbroken continuity through pre-Silurian epochs, amidst all the changes of the Carboniferous, Triassic, Oolitic, Cretaceous, and more recent geologic ages, with that mutability as an essential characteristic which they are now seen to display, and yet that they should have undergone little or no alteration, seems too incredible to be seriously entertained.

It is impossible to say that they have preserved their primitive forms by reason of their existence in unchanging environments. The very reverse of this must have been the fact; and, even now, such organisms are to be met with abundantly all over the face of the earth, and in the most diverse situations.

But, in accordance with the views advanced in this work, the present-day existence of these organisms may be fully explained, and is just what might be expected—if they are ever seething up anew by Archebiosis and by Heterogenesis. These views, moreover, leave us free to admit that their mutability is extreme, and that their habitats are most diverse and world-wide—as all who are well acquainted with such organisms should be free to acknowledge.

This very circumstance of *the wide-spread distribution over the earth of similar lowest types of life* affords another test of the relative validity of the two views. The facts brought forward in this work seem to show that the intrinsic molecular composition and properties of the different varieties of living matter have much more to do with the forms and structures of lowest organisms than mere differences in their environment. Under the dominating influence of 'organic polarity,' the several forms arising by Archebiosis and Heterogenesis seem to unfold more or less immediately into such and such simple organisms of common type.

Then, again, the facts and arguments furnished by heterogenesis, coupled with Herbert Spencer's views concerning 'physiological units,' seem capable of affording a better explanation than has hitherto been forthcoming of the *sudden and abrupt variations in higher plants and animals* which are well known to occur—variations that have been spoken of as 'discontinuous' by Bateson, and as 'mutations' by De Vries, and which are often so marked as to constitute sudden origins of actual new species of animals or plants. These phenomena are, perhaps, the nearest approaches possible among higher organisms to the still more marked transformations described in this work as occurring among lower organisms. Yet

such abrupt 'mutations' are now admitted by biologists to be both real and frequent—"proved up to the hilt" as an authoritative writer has lately said.

We come next to other facts relating to higher organisms, and having reference not only to their distribution over the face of the earth at the present day, but also to the distribution of their fossil remains in the different strata constituting the crust of the earth.

The new views would seem to diminish, to an enormous extent, many of the difficulties at present existing in regard to the more limited *geographical distribution of higher plants and animals.* Darwin with his usual candour said[1]: "Undoubtedly there are many cases of extreme difficulty in understanding how the same species could possibly have migrated from some one point to the several distant and isolated points where now found." He then went on to say: "Nevertheless the simplicity of the view that each species was first produced within a single region captivates the mind. He who rejects it, rejects the *vera causa* of ordinary generation with subsequent migration and calls in the agency of a miracle." Of course in this latter statement Darwin was referring to an upholder of the obsolete view, which he so successfully combated, of the supposed 'special Creation' of species. But so far from appealing to "miracle," in postulating multitudinous centres of origin as possible, I appeal to the uniformity of natural phenomena in space and in time.

This last word brings us, indeed, to another and a related test for the relative validity of the two views now under consideration, by a comparison of the aid they may afford in tracing, what has been termed, *the ancestral history of organisms from a study of their fossil records.* It was formerly thought by geologists that rocks containing similar fossils were to be regarded as contemporaneous formations. But this view was traversed by Huxley in a celebrated presidential address to the Geological Society in 1862, wherein he advanced the strongest evidence in favour of the position that "similarity of organic contents cannot possibly afford any proof of the synchrony of the deposits which contain them; on the contrary it is demonstrably compatible with the lapse of the most prodigious intervals of time, and with interposition of vast changes in the organic and inorganic worlds between the epochs in which such deposits were formed." And, in illustration of this lack of synchrony (or identity of date) he said, "For anything that geology

[1] "Origin of Species," 6th ed., p. 320.

or palæontology are able to show to the contrary, a Devonian fauna and flora in the British Islands may have been contemporaneous with Silurian life in North America, and with a Carboniferous fauna and flora in Africa." Nobody, now, contests these views, nor believes, as was formerly the case, that any advance in the successional series of living forms must have occurred simultaneously in different parts of the world, and, as a consequence, that strata in which similar fossils are found must have been contemporaneous formations.

But, if the old view is incorrect, and if very similar faunas may be found imbedded in rocks whose formation has been separated by long-drawn ages of time, the difficulties besetting the biologists, in regard to time, are just as great or even greater than those, in regard to space or geographical distribution, to which Darwin referred in the preceding quotation.

But the distribution of similar organisms through geological strata in widely different parts of the world, and in formations belonging often to widely separated geological ages, will certainly be also capable of receiving a much better explanation if, instead of supposing that all organic forms have developed and spread from one single centre, we are prepared to recognise that there have been multitudes of independent centres with wide developments in and from them. It should never be forgotten that, as G. H. Lewes said,[1] "The link which unites all organisms is not always the common bond of heritage, but the *uniformity of organic laws acting under uniform conditions*." And if, through all the life-evolving period of the history of our globe, the progress of 'organisation' seems to have been essentially similar (so that development may have many times gone along more or less similar lines) this seems readily explicable by the consideration that living things, both as regards their origin and their subsequent differentiation or development, are the immediate products of ever-acting natural laws and material properties.

And that such an essential similarity has existed may be gathered from the following authoritative statements made by Prof. Huxley in the address above referred to. He said: "There are two hundred known orders of plants; of these not one is certainly known to exist exclusively in the fossil state. . . . The positive change in passing from the recent to the ancient animal world is greater, but still singularly small. No fossil animal is so distinct

[1] "Fortnightly Review," 1868, vol. i., p. 373.

from those now living as to require to be arranged even in a separate class from those which contain existing forms. It is only when we come to the orders, which may be roughly estimated at about one hundred and thirty, that we meet with fossil animals so distinct from those now living as to require orders for themselves; and these do not amount, on the most liberal estimate, to more than about 10 per cent. of the whole."

But the truth of the new views is still more strongly shown by the light which they are capable of throwing upon another closely related palæontological problem of the greatest importance, which has hitherto proved a stumbling-block to biologists and evolutionists alike—that is, *the fact of the existence of 'persistent types' of life through long geologic ages up to the present day.* As Huxley and others have shown, this long persistence of similar organic forms has been met with both among animals and among plants of comparatively high organisations. Certain genera of Mollusks, for instance, are said to "have persisted from the Silurian epoch to the present day with so little change that competent malacologists are sometimes puzzled to distinguish the ancient from the modern species." [1] This persistency is assumed by some to be due to the slow rate of change among such organisms, or to their having passed into a rigid (as opposed to a plastic) condition; and they would have us believe that such organisms have been perpetuating their kind, in the same likeness, through this long succession of geologic ages.

An excellent means of refuting this supposed explanation is fortunately open to us, seeing that a similar persistency, through many geologic ages, is known to obtain for two sets of very low organisms closely akin to those which the writer has shown to take origin so frequently by heterogenesis, and which are also notorious for their high degree of variability. I refer to Foraminifera (chambered Amœbæ) and to Diatomaceæ—very low types respectively of the animal and of the vegetable kingdoms. It is the siliceous envelopes of these low unicellular organisms, with their characteristic shapes and markings, that are preserved in the fossil state, and which thus permit of an accurate comparison being made of ancient and existing representatives of these simple forms of life.

Concerning the former organisms Dr. Carpenter wrote,[2] "there is no evidence of any fundamental modification or advance in the

[1] "Proceed. of Royal Institution," vol. iii., p. 151.
[2] "Introduction to the Study of the Foraminifera," 1862, p. xi.

Foraminiferous type from the paleozoic period to the present day." Similar types and similar varieties from those types are, he said, to be met with in geological formations existing as far back as the triassic rocks.[1] And in reference to the same subject Pritchard[2] spoke as follows: "It may be generally stated that the relative number of identical fossil and recent species is much greater in this family of Foraminifera than in any other known; and specific forms have continued from the Mesozoic Era until the present day, so connecting, as by an unbroken chain, the fauna of our own time and that of almost countless ages past." Yet the variability of these organisms is extreme, as may be gathered when Dr. Carpenter says: "The only natural classification of the vast aggregate of diversified forms which this group contains, will be one which ranges them according to their direction and degree of divergence from a small number of principal family types."

Turning now to the Diatomaceæ, whose extreme variability is no less notorious, Dr. Gregory[3] wrote as follows in reference to the agreement of recent and fossil forms: "I have no hesitation in saying that I believe all the forms in the Ægina clay marl, which is the oldest Diatomaceous deposit yet described, will be found living on our coast.[4] . . . It may also be observed that of all the forms figured by Ehrenberg from more recent strata . . . the great majority are perfectly identical with existing Diatoms." He subsequently said: "Taking these facts into consideration, I am led to believe that we have no evidence that any species of Diatom has become extinct, as so many species, and even genera and tribes, of more highly organised beings have done." Sir Joseph Hooker, Pritchard, and other writers also bear witness to the cosmopolitan distribution of these organisms. And the fact that they show themselves in the most varied regions with identical anatomical characters, seems to indicate pretty conclusively that the intrinsic properties and varieties of the living matter of which they are composed (that is, of their 'physiological units'), have more to do

[1] But if certain low organisms developed into Foraminifera in remote geologic ages, there is no reason why they should not develop in the present day into essentially similar forms; and variation may now tend to manifest itself in the same fashion as it did formerly, owing to the fact that the causes (both intrinsic and extrinsic) leading to this variation are essentially similar.

[2] "History of Infusoria," 4th edn., p. 232.

[3] "Proceed. Roy. Soc. Edin.," 1856–7, p. 442.

[4] The stratum at Ægina is said to belong either to the chalk formation, or to the oldest tertiary or Eocene beds.

with their forms and structures than any differences in the 'conditions' under which they have been born and have lived—a view which is supported by the fact of the similarity of the forms of Diatoms and of Foraminifera that existed upon the surface of the earth or in its oceans in remote ages with those living at the present day.

But are we to assume that these and other low organisms to which reference has been made have had an unbroken lineal descent through millions and millions of years since they first came into existence? This is the commonly received notion, in accordance with the view that Archebiosis no longer occurs, and that Heterogenesis is equally to be denied.[1] In view of the extreme variability of all these organisms, and in face of the fundamental principles of Evolution the supposition will, perhaps, to most persons prove absolutely incredible. If lineal descent without alteration had been the rule in past ages and up to the present day, we might at least expect now to find such evidence of this continuity as would be represented by *fixity of habitat*. Fixity of habitat is, however, notoriously non-existent among these lower forms of life. Speaking of Rotifers, Pritchard says[2]: "One remarkable circumstance must be borne in mind by the animalcule hunter. If he happens to remember a pond where some rare species abounded last year, let him not again turn thither in search of it, as the chances will not be in his favour. These creatures rarely exist in the same water during two successive years. The reasons for this are not easily ascertainable. The remark is equally applicable to Volvox and the Desmidiæ. The search will be most productive if prosecuted on new ground." Statements of this kind have also been made by others and are fully borne out by my own experience.

Ralfs, for instance, in reference to some Desmids, says[3]: "The *Staurocarpus cœruleus* is not uncommon near Penzance, is generally in large quantity where it occurs, and from its peculiar colour cannot escape detection; on these accounts I made it a principal subject of my observation. Although I have yearly gathered it in several pools, and the sporangia are always abundantly produced, I have particularly noticed during five or six years' observation

[1] Huxley, for instance, said, "In view of the facts of geology, it follows that all living animals and plants are the lineal descendants of those which lived long before the Silurian epoch."

[2] *Loc. cit.*, p. 655.

[3] "The British Desmidæ," 1848, p. 14.

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that it has never in a single instance reappeared in the same pool. At Dolgelly, where also in some years it is common, I met with the same result, with a single exception, when I gathered it in one pool for two successive years. I have noticed the same fact with regard to *Zyngema curvatum*, and I believe it holds good in regard to most if not all the other Conjugatæ."

In the face, then, of this absence of continuity of habitat in the present, and of the frequent extreme variability of the lower forms of life—some indications of which have been given in the foregoing quotations from F. Cohn, Carpenter and others, together with what has been said as to the variability of Algæ generally (pp. 289–291)—how is it possible to accept the notion of direct lineal descent without change, as the explanation of "persistence of types"?

As I long ago pointed out[1] persistence of low types of life is much more explicable on "the assumption of successive evolutions of more or less similar forms from similar starting points, under the influence of like conditions, than on the assumption that such changeable forms should have produced their like through such vast and unrealisable epochs of time." Persistence of types among lower forms of life is, in fact, to be expected in accordance with my views, seeing that the living things that have been constantly arising by Archebiosis and Heterogenesis have been the immediate products of ever acting material properties or natural laws—the same in all times, however much or little the environing conditions may have varied from age to age.

Thus, the continued existence of low types throughout the geologic strata from the Silurian system upwards; and, among higher types, the constant admixture of previously known forms with others altogether new, will be found quite consistent with the notion of a continual surging up through all geologic time of freshly evolved, lower forms of life—representatives of which, as they become more and more highly organised, mix, in successive epochs, with those of their predecessors which still remain. In this way there may have been produced in successive geological ages, in different parts of the earth's surface, multitudes of what have been called "trees of life" branching out into animal and vegetal forms of almost inconceivable variety. Many of these trees, including all or most of their branches, may have died out during the many vicissitudes of the earth's surface and the long

[1] "The Beginnings of Life," vol. ii., p. 616.

lapse of ever-fruitful ages; though the descendants or ultimate ramifications of other of these trees—dating back to quite different and perhaps far-distant epochs—may still survive upon the earth's surface. How far, however, those roots from which the existing higher forms of life are derived, may have extended back into the depths of geologic time, we are utterly unable to estimate.[1]

It is only natural to expect in accordance with these views that, while more or less similarity would be likely to exist between the lower forms of life which have appeared at different periods of the earth's history, more and more divergence might be encountered amongst such higher aquatic or aerial types whose ancestors may have lived through long geologic ages. Among such forms, considerable diversity may be induced by the operation of the ordinary factors of evolution. So that if the descendants of similar organisms (derived perhaps from totally independent stocks) have been exposed to notably different external conditions in different ages; or if in any of them modifications have otherwise arisen, the forms ultimately produced along such lines of development may be widely different from one another, although belonging to similar types.

And at different periods in the earth's history, specialisations, now of one type and now of another, have been more and more manifest or dominant. In the Silurian epoch, strange crustacean Trilobites abounded in all the seas. In the Devonian epoch fishes of a remarkable structure were most plentifully represented; while in the earlier Carboniferous period the cup-like Encrinites existed in abundance, though in the later portions of this epoch they were altogether thrown into the shade by that vast tropical vegetation from which we now derive our supplies of coal. In the Oolitic period, or so-called 'age of reptiles,' we have a most remarkable abundance of Saurian forms, and the Amphibian type reached its highest development. Huge Ichthyosauri and Plesiosauri swam in the lakes and rivers, whilst strange and gigantic winged Lizards mounted into the air. In the later Tertiary period

[1] Cope in his work, "The Primary Factors of Organic Evolution" (1896), dwells upon the fact "that the phylogenetic lines have not been continuous"; he says, "the point of departure of the progressive lines of one period of time has not been from the terminal types of the lines of preceding ages, but from points further back in the series." And again he says, "Many lines of variation have been at one geologic period and another discontinued. It is also true that certain divergencies from the main lines have appeared. . . . Such variations do not seem to have had any material effect on the general Course of Evolution" (pp. 172–222).

we find the Mammalian type exhibiting the greatest divergence from previously-existing forms; first, by the appearance of innumerable, huge Mastodons, Megatheriums, and other unwieldy denizens of the ancient forests and plains; and subsequently, by the gradual modification of one of the ramifications of the Quadrumanous order, into those beings from whom primæval Man himself seems to have been evolved.

These several types of life, however, which have from time to time become more and more specialised, need not in any sense represent the members of one progressive series. They may be rather the products of different evolutional processes—divergencies, taking place now in one direction and now in another, though reproducing fundamentally similar types of organisation. The lack of synchrony in the formation of similar fossils to which Huxley called attention, and their separation often by vast periods of time, might thus be in a measure explained.

Our knowledge of the various living forms which have existed in past ages is still of the most fragmentary character. Such as it is, however, the record seems to show very plainly that there has been nothing approaching to a continuous progression terminating in the Mammalian type. Vertebrata in the form of fishes, as high as any existing at the present day, have been in existence since the time when the upper Silurian rocks were deposited. While at different intervening periods in the earth's history, now one, now another of the invertebrate forms of life have been in the ascendant, associated, perhaps, with representatives of some highly developed and divergent branch of the vertebrate tree. Till at last—as it were accidentally—at the summit of one of these diverging branches, some of the branchlets pertaining to the quadrumanous order began to undergo modifications which terminated in the evolution of the immediate ancestors of the primæval representatives of our race.

It is, therefore, needless to regard all preceding forms of life as belonging to types lower than our own, or to suppose that they have been the necessary precursors of our advent.

There is indeed no small amount of evidence, deducible from the history of the life of the globe antecedent to the advent of Man, tending to prove that many of the above-mentioned developmental divergencies cannot be regarded as constituting so many necessary preliminary series. The palæontological records, so far as they have been discovered, would rather encourage a belief that

we happen to live during one of those great phases in the earth's history in which an aberrant type, having within itself vast and altogether peculiar capacities for improvement, has, on account of the high development of these capacities, overrun the earth. Those mysterious powers and natural tendencies, which formerly sufficed to produce the great fish-like lizards and crocodiles, and which, among birds, have expended themselves in the perfection of an elaborate respiratory system and in the production of related changes in their integumentary system and organs of locomotion, seem, in the case of Man and of the race from which he has been developed, to have been expended in the production of much less obvious external changes, although these have been accompanied by the most important internal changes, leading to the gradual elaboration of the Brain, or principal Organ of Mind.

An increased development of the brain, however initiated, and even when it gave to primæval Man mental powers very slightly in excess of those of the man-like apes, would, after a time, as Wallace has ably shown,[1] almost inevitably tend to give him that power over natural products and forces which in the course of ages has enabled him to make these forces subservient to his own wants in a gradually increasing degree.

The facts and views brought forward in the present work will be found to have a very important bearing upon another problem of great speculative interest, namely, the question of *the time needful for the Evolution of all the Forms of Life that have ever appeared upon the Earth*, and should not be without influence in bringing much more into harmony, than is at present the case, the views entertained by physicists, geologists, and biologists respectively, as to the probable duration of life upon our globe.

The actual age of the globe, and also the time that must be supposed to have elapsed since the first appearance of living things upon its surface, has given rise to a considerable amount of discussion since Sir William Thomson (now Lord Kelvin) in 1862 first endeavoured to show in a paper[2] on "The Secular Cooling of the Earth," that great limitations had to be put upon the enormous demands for time made by Sir Chas. Lyell and his immediate followers in accounting for all the series of changes on the surface of the earth that come within the ken of the geologist.

The very positive expression of Lord Kelvin's views in this

1 "Contributions to the Theory of Natural Selection," 1870, p. 319.

2 "Trans. of Roy. Soc. Edin.," vol. xxiii., p. 157.

paper caused, for a time, some consternation among both geologists and biologists. He returned to the subject in other communications in 1865, in 1868, and again in 1899 arguing from certain physical and astronomical data. His views as to the amount of time that could be conceded as possible to have elapsed since the consolidation of the earth have varied in these different communications, though his tendency has been to limit it more and more, till in his latest utterance on this subject[1] he put it as "more than twenty and less than forty millions of years, and probably much nearer twenty than forty."

His views and reasonings have been criticised by other physicists, and especially by Professors Perry and George Darwin, who have attempted to show, with much success, the uncertain nature of the data and assumptions upon which Lord Kelvin's conclusions have been founded.

Moreover, the time limits assigned by him have been considered altogether inadequate by geologists and biologists alike. The most authoritative demurrer on the former side may be found in Sir Archibald Geikie's address[2] to the Geological Section of the British Association in 1899, in which the whole subject is discussed in a full and interesting manner. He considered that nothing short of 100 million years "would suffice for that portion of the history which is registered in the stratified rocks of the crust" (*loc. cit.*, p. 500)—and, of course, very much more for the total duration of life upon the planet. On the other hand Professor Poulton, three years previously, in his address[3] to the Zoological Section of the British Association, had dealt in an elaborate manner with the problem from the biological standpoint. He seemed to consider that a period "several times" as long as that which had previously been indicated by Geikie in 1892 would be needed to account for the evolution of all the forms of life upon the globe from the Cambrian epoch onwards. The pre-Cambrian period, in his estimation, may have been three or four times as prolonged, and would certainly be double as long, as the subsequent ages during which the whole stratified crust of the globe had been laid down and all the forms of life known to us had been evolved. This view as to the probable vast duration of the pre-Cambrian ages is shared by geologists generally, and is also in accord with

[1] "Phil. Mag.," January, 1899, p. 75.
[2] "Nature," September 21, 1899.
[3] *Ibid.*, September 24, 1896.

opinions expressed by such leading evolutionists as Darwin, Herbert Spencer, and Huxley. It is based upon the fact that the fauna of the Cambrian age includes fossil remains of five out of the six animal sub-kingdoms, namely, the Protozoa, Cœlenterata, Annuloida, Annulosa, and Mollusca—representatives in fact of all the sub-kingdoms save the Vertebrata, which, in the form of fishes, first appear in the Silurian strata.

Taking the data made use of by Poulton, the time he thought needful to demand amounted, as Sir Edward Fry has shown in an interesting article on "The Age of the Inhabited World and the pace of Organic Change," to no less an astounding total than 2,700 million years. On the basis of 100 million years from the Cambrian rocks onwards, however, the sum would still work out to 600 million years as the biological estimate for the duration of life upon the globe.[1]

As I have said, the first announcement of Lord Kelvin's conclusions undoubtedly came as a shock to biologists and geologists alike. Thus Darwin, writing to Wallace on April 14, 1869, said: "Thomson's views of the recent age of the world have been for some time one of my sorest troubles." For even the widest limit that Lord Kelvin at that time was prepared to concede for the whole age of the world was altogether at variance with what would be required for the mere duration of life upon its surface, in accordance with Darwin's views as to the very slow means by which new species had been evolved, and his supposition of a single starting-point for living matter at some one particular time, and in some one particular place on the surface of the earth.[2]

Another of the principal reasons that made it necessary for Darwin to make extremely large demands upon time is to be found in his view that low forms of life change, or become modified, less quickly than the higher forms.[3] This same doctrine was also most strongly enforced by Poulton in the before-mentioned address. He said:[4] "Undoubtedly a study of all the available evidence points very strongly to the conclusion that in the lower grade, sub-grades, and phyla of the animal kingdom, evolution has been extremely slow as compared with that in the higher."

But these views as to the rate of change in lower organisms are based upon the most questionable data. It is obvious that Poulton

[1] "Monthly Review," December, 1902, and January, 1903.
[2] "Origin of Species," sixth ed., p. 429.
[3] *Loc. cit.*, p. 346. [4] *Loc. cit.*, p. 506.

was in the main influenced by the facts known concerning "persistent types," among the lower forms of life, existing in geological strata from the Cambrian and Silurian periods onwards. He, indeed, distinctly intimates that the longer the persistence through geologic ages of any given type of life the greater would have been the time needful for its origination.[1]

What I have already said concerning "persistent types," however, will have shown that a totally different interpretation may be put upon such facts, when once the doctrine of heterogenesis is admitted. It certainly would no longer be needful to assume, with Darwin and with Poulton, that the rate of change is slow among low as compared with higher forms of life. Such a conclusion is directly opposed to a large mass of other evidence, and is, I believe, absolutely the reverse of the truth.

Such a view is, moreover, far from being shared by other eminent biologists. Thus Adam Sedgwick, in his address to the Zoological Section of the British Association in 1899, said that the facts of palæontology when closely scrutinised lend support to the view that "variation was much greater near the dawn of life than it is now, and heredity a correspondingly less important phenomenon"; while Prof. Hickson on a similar occasion in 1903 gave expression to very similar views. He went even so far as to say, "that in the earliest stages of evolution the condition of extreme plasticity and ready response to changing external conditions were necessary for the survival of the species." Several interesting facts and quotations, in the same direction, are also cited by Sir Edward Fry in the before-mentioned article,[2] while Charles A. White calls attention to many other important palæontological facts having a like significance.[3]

Thus, if instead of believing with Darwin that "all the living forms of life are the *lineal* descendants of those that lived long before the Cambrian epoch," and that "all the organic beings which have ever lived on this earth may be descended from some one primordial form," it should be admitted that life originally started from multitudes of centres (as the uniformity of natural phenomena would demand); that from the earliest stages of the earth's history up to the present time new starting points of simplest forms have been ever taking place all over the surface

[1] *Loc. cit.*, p. 509.

[2] "The Monthly Review," January, 1903, p. 81.

[3] "The Smithsonian Report," 1901, p. 638.

of the earth, we may see, not only how many of the facts concerning "persistent types" may be explained, but also how the time needed for the whole evolution of life upon the globe may have been far less prolonged than biologists have hitherto supposed. The naturalist would be no longer bound to look upon all animals and plants as members of a single imperfect series. The routes that have given rise to all the known forms of life may have been many, and several of the evolutional series may have been developing not only contemporaneously, but also to some extent similarly from new starting points in successive geological epochs. May not the existence of Amphioxus at the present day in our seas be an evidence of this? Are we to suppose that this minute organism which is regarded as "the ancestral form of all vertebrates," has come down to us unchanged from pre-Silurian epochs? Cope (*loc. cit.*, p. 99) speaks of a fish of the Carboniferous age as of a type from which all fishes may have sprung, "although the genus as now known has not sufficient antiquity to claim this place." But when he adds, "It may be a near descendant of the Amphioxus," he clearly indicates his belief in the amazing persistence of this form of life. But who is to say that it has not originated from some annelid or some tunicate during a comparatively recent epoch? A similar questionable persistence is postulated by Poulton for Peripatus—though this organism has also never been found in the fossil state.

The foregoing brief survey should suffice to show the reader how much better multitudes of well-known facts can now be explained, and how absolutely irreconcilable many of them are with the old point of view. We have seen that this holds good in reference to the following classes of facts:—

(1) Concerning the present day existence of multitudes of the lowest organisms.

(2) Concerning their wide-spread distribution over the surface of the earth.

(3) Concerning abrupt variations ('mutations') in higher plants and animals.

(4) Concerning the geographical distribution of higher plants and animals.

(5) Concerning the distribution of the fossil remains of higher animals through the crust of the earth.

(6) Concerning the existence of recurring or so-called "persistent types" of life.

(7) Concerning the time that may have been needed for the evolution of all the forms of life that have ever appeared upon the Earth.

This great accumulation of evidence in favour of the new views, in harmony as it is with the writer's actual observations and experiments, is still further strengthened by the fact that the new view does not (as the old view does) postulate any inexplicable departure from the uniformity of natural phenomena. It assumes that the forces of nature and material properties have ever remained the same, and that new births of living matter have ever been taking place on the surface of the earth since the time when such processes first became possible.

Having arrived at such a conclusion our thoughts may not unnaturally wander away beyond the bounds of our own solar system—to the universe at large, with its inconceivable hosts of stars and nebulæ: that is, of systems like our own with their planets and satellites, and of other such systems in all stages of evolution. And the question of the existence of "Life in the Universe," beyond the pale of our small planet, cannot fail to suggest itself as one of absorbing interest. This problem has recently been considered ("Harper's Magazine") by Simon Newcomb, one of America's most famous astronomers. From facts stated, he feels justified in coming to the conclusion that "the number of worlds which, so far as we know, may be inhabited are to be counted by thousands and perhaps by millions." In arriving at such a conclusion he, of course, assumes, as in scientific predictions generally, the uniformity of natural phenomena. Looking to the countless hosts of stars and their related planets, he says: "In a number of bodies so vast we should expect every variety of conditions as regards temperature and surroundings," so that, if we suppose "the special conditions which prevail on our planet are necessary to the highest forms of life, we still have reason to believe that these same conditions prevail on thousands of other worlds." This conclusion is quite compatible with the views expressed in this work.

As I have endeavoured to show, there are good reasons for the conviction that the same Forces which are now in action within and around us, have been and are constantly operative throughout the whole universe—everywhere producing the most uniform and complex results which combine in testifying to the existence of one supreme and all-pervading Power of which these results are the phenomenal manifestations.

APPENDIX

ON THE GREAT IMPORTANCE FROM THE POINT OF VIEW OF MEDICAL SCIENCE OF THE PROOF THAT BACTERIA AND THEIR ALLIES ARE CAPABLE OF ARISING DE NOVO[1]

THE intimate relations that have been proved to exist between microorganisms and so many of the contagious or communicable diseases, and the discovery that in many cases the organisms in question act as the veritable contagia by means of which such diseases are spread from person to person, have exercised such an enormous influence over the minds of medical men that they have led to the almost universal establishment of ultra-contagionist views in regard to all these diseases. Just, it is thought, as organisms are propagated only, and do not arise *de novo*, so the reigning doctrine in medicine has been for some time, and still is, that contagious diseases are propagated only and never arise *de novo*. The breaking down of the prejudice in regard to organisms, by showing that they can originate independently of pre-existing microorganisms of like kind would of necessity exert a powerful influence over medical doctrines, and would pave the way for the admission that contagious diseases may also arise *de novo* instead of being only disseminated by contagion. Short of a proof of this kind there seems less chance of any such widening of doctrine being brought about.

To show the kind of feeling that exists I may recall the fact that one of the most distinguished physicians in this country not very long since said:[2] "If I can trace contagion in a very large number of the so-called specific diseases I consider it more reasonable to assume contagion in the minority than look about for another cause." And he went on to say that, as many of these diseases

[1] This article first appeared in the columns of "The Lancet," for October 31, 1903; but the bearing of my views on medical science has been so much misrepresented, that I have thought it well to reproduce it here.

[2] Letter from Sir Samuel Wilks, "British Medical Journal," December 23, 1893.

are associated with the growth and multiplication of "living specific organisms" a belief in the *de novo* origin of these contagious diseases would "imply also a belief in spontaneous generation." This latter notion is undoubtedly very common and has been one of the principal causes that has stood in the way of a belief in the possibility of the *de novo* origin of a contagious disease. On this account, therefore, the proof of the heterogenetic origin of Bacteria becomes a matter of very great importance for medical science. Still, the conclusion above drawn, notwithstanding its prevalence and great influence, does not of necessity follow, as I shall hope to show.

We all know that common Bacilli and Micrococci are constantly making their entry into the body through the intestinal and the respiratory mucous membranes, and thence are gaining access to the lymphatic system. So that for the origin of this or that specific disease it may not be at all necessary that a *de novo* origin of microorganisms should take place. Under the influence of unhealthy local or general conditions the common microorganisms thus entering into the body may possibly be made to take on new properties and be, in fact, converted into one or other of the so-called "specific" or "pathogenic" microorganisms.

It is needless for me to cite in support of this latter possibility the vast array of facts now known concerning the variations and interchangeability of form that may be brought about in these microorganisms by changes in the media and conditions to which they are subjected, and the still more important variations in function and in the chemical processes associated with their growth and multiplication that may be similarly induced. This is so notoriously the case that many writers, such as Billroth, Nägeli, Warming, Cienkowski, Ray Lankester, Kopf, and others have regarded these microorganisms as mere phases or varieties, modified by external conditions, of one and the same, or of but a very few distinct species. Many facts of importance in this relation, in connection with pathogenic Bacteria and their possible derivation from non-pathogenic forms, are matters of common knowledge.

There is one case, however, so pertinent to the present inquiry and of such great importance in itself that some details will prove most useful. I allude to our present knowledge concerning certain experimentally produced diseases in lower animals included under the name *Septicæmia.* Two of these forms of septicæmia have

been investigated experimentally with the greatest care.[1] One of them, known as "Davaine's septicæmia," may be originated in a previously healthy animal by injecting two or three drops of putrid blood (bullock's or that of any other animal) into the subcutaneous tissue of a rabbit. The animal dies in from twenty-three to twenty-five hours, Bacilli in myriads and of a distinctive kind existing, even during life, in its blood. This constitutes the origin of a disease which proves to be contagious, and so much so that it can be propagated from animal to animal by even the millionth part of a drop of blood, and thus on indefinitely. The other form is known as "Pasteur's septicæmia" and it is producible at will in this fashion. Let two or three drops of putrid bullock's blood from the same stock be this time injected into the peritoneal cavity of a rabbit, rather than into its subcutaneous tissue, and now a different form of disease is established, though one which is also contagious and equally constant in its characters. In this case the animal does not die so rapidly, and while Bacilli swarm in the fluids of the peritoneal cavity within twenty-four hours they are not to be found at the time of death in the blood, though they appear there and throughout the body in the course of a very few hours after death. Another difference between these two varieties of septicæmia is that though the latter form of the disease is also contagious and capable of being propagated indefinitely by the inoculation of a minute quantity of the peritoneal fluid, yet this fluid contains a contagium which is nothing like so virulent as that contained in the blood in "Davaine's septicæmia." Instead of one millionth of a drop, which is adequate for contagion in this latter case, it is found that about a drop of the infecting peritoneal fluid is needed in the case of "Pasteur's septicæmia."

But now another and even more important point has to be mentioned. It is this. We are told that absolutely no difference can be detected between "Pasteur's septicæmia" and that which is initiated after the manner of Burdon Sanderson by injecting a small quantity of a germ-free chemical irritant into the peritoneal cavity or into the subcutaneous tissue of a rabbit. Difference in the site of introduction of the mere chemical irritant produces no difference in the disease. Its action in either situation is to set up a most virulent inflammation, the fluids of which speedily teem

[1] See "Report on Experimental Investigations on the Intimate Nature of the Contagium in Certain Acute Infective Diseases," by G. F. Dowdeswell, "British Medical Journal," July 19, 1884, pp. 101–8.

with Bacilli, and in every particular the malady so induced has been shown by other pathologists exactly to resemble "Pasteur's septicæmia." Burdon Sanderson's words concerning the actual *de novo* production at will of this contagious disease are as follows:[1] "If a few drops of a previously boiled and cooled dilute solution of ammonia are injected underneath the skin of a guinea-pig a diffuse inflammation is produced, the exudation liquid of which is found after twenty-four hours to be charged with Bacteria. . . . Other chemical agents will lead to the same results and *always under conditions which preclude the possibility of the introduction of any infecting matter from without.*" Elsewhere[2] the same investigator referred to experiments which were made about the same time in order to throw light upon the cause of the appearance of Bacteria in certain peritoneal exudations and to ascertain whether or not their presence was to be considered as "a mere result of the intensity of the peritonitis." He says: "To determine this, experiments were made during the following month (May, 1871) which consisted in inducing intense peritonitis by the injection not of exudation liquids but of chemical irritants, particularly dilute ammonia and concentrated solution of iodine in hydriodic acid. As regards the ammonia, precautions were taken to guard against contamination by boiling and cooling the liquids as well as the implements to be used immediately before injection. In the case of the solution of iodine this was, of course, unnecessary. In every instance it was found that the exudation liquids, collected from twenty-four to forty-eight hours after injection, were charged with Bacteria, whence it appeared probable that the existence of these organisms was dependent, not on the nature of the exciting liquid by which the inflammation was induced, but *on the intensity of the inflammation itself.*"

The organisms in the cases where germ-free chemical irritants have been employed have therefore come from the previously healthy body of the animal experimented upon—either by way of heterogenesis or by the waking up of previously "latent germs" of common microorganisms, instead of being, as in other experiments, modified descendants of the common putrefactive organisms contained in the bullock's blood. But the point of importance is that in either case, under the influence of local inflammatory

[1] "Transactions of the Pathological Society," 1872, pp. 306–8. (No Italics in original.)

[2] "Transactions of the Royal Medical and Chirurgical Society," 1873, p. 365.

processes of great intensity, such common organisms have been converted into specific or "pathogenic" microorganisms, capable henceforth of preserving their specific characters and of "breeding true" in suitable media.

Dowdeswell, who has many times repeated these experiments for the production of both varieties of septicæmia, points out that the form named after Pasteur corresponds with what Koch has termed "malignant œdema," and that the bacillus which characterises it is an extremely common form often found in the outside world. Still, when the germ-free chemical irritants are used, he declares his belief that infection from without is precluded and that the Bacilli "originated from within the animal organism."[1] In regard to the production of the form named after Davaine it appears that putrid blood sometimes fails, especially in winter. Some particular stage of the putrefactive process seems necessary. Nothing more than this is proved, though Dowdeswell assumes, without adducing a vestige of proof, that the organism characterising this form of the disease in some way gets into the putrefying blood owing to "atmospheric contamination." But this is a mere unsupported guess. The evidence tends to show that it is one of the many forms developed at a certain stage in the putrefying blood, which is capable of infecting the system through the subcutaneous tissue, but not when introduced into the peritoneal cavity. This Bacillus of Davaine's septicæmia is, according to Dowdeswell, a so-called "specific organism" whose characters he has minutely described;[2] and it is not known to exist in the outside world apart from putrid blood in certain stages of change.

This production of two different forms of septicæmia by the inoculation of some of the same putrid material into different sites is a matter of the greatest importance. The putrid blood under the skin gives rise to one form of specific microorganism and contagious disease, while two or three drops of the same putrid blood introduced into the peritoneal cavity of a similar animal give rise to swarms of a different organism and the development of another contagious affection. The differences in the inflammatory processes in the two situations are capable, that is, of transforming some common microorganisms into two quite different specific bacilli; while the germ-free chemical irritants, with even more

[1] *Loc. cit.*, p. 104.

[2] "Journal of the Royal Microscopical Society," 1882, vol. ii., p. 310, and the "Quarterly Journal of Microscopical Science," 1882, p. 66.

certainty, give rise to one and the same contagious affection whichever may be the site of their introduction.

These are all facts the importance of which can scarcely be over-estimated. They afford a sort of beacon light capable of illuminating the obscurity surrounding the origin of many other contagious diseases. A few examples will suffice to show the mode in which they may prove helpful in overcoming difficulties which are commonly thought to stand in the way of a belief in the *de novo* origin of other of these contagious diseases.

In *Typhoid Fever* we have one of the commonest of contagious affections about the possibility of whose *de novo* origin the greatest difference of opinion has long existed, though of late the ultra-contagionist view has been decidedly gaining ground here as in other directions. But there are, perhaps, some still who, while admitting contagion and the common spread of the disease through contaminated water or milk, or by other agencies, would be inclined to agree with Rodet and Roux that this disease may originate *de novo*, and that the typhoid Bacillus of Eberth is merely an altered and virulent form of the common Bacillus of the colon.[1] The researches of these observers have tended to show that this latter Bacillus, which commonly exists in the human intestine without harmful results, can become highly virulent and infective, under certain conditions, when introduced into water. Hence they conclude that not only typhoid dejections but simple fæcal pollution of water may produce typhoid fever in those who drink it. This is undoubtedly a very important point and one which hitherto has not been adequately taken into account by those who have attempted, as they thought, to trace the source of contagion in many cases of typhoid fever. Because there has been pollution of a water source by a man suffering from diarrhœa, and typhoid has been produced in persons drinking such water, it must not be assumed without proof that the man who polluted the water was suffering from typhoid fever. As bearing upon this view of Rodet and Roux it is interesting to note that typical enteric lesions in the small intestine have been artificially induced in lower animals by R. Row[2] of Bombay by intoxicating them with the products of the Bacillus coli communis; and that the lesions

[1] As to the degree of the relationship between these forms and the frequent difficulty in distinguishing one from the other, see Crookshank's "Bacteriology and Infective Diseases," Fourth Edition, 1896, pp. 344–46.

[2] "Transactions of the Bombay Medical and Physical Society," vol. iv., No. 5.

thus produced have been even more marked than when similar animals have been intoxicated with the products of the more specialised Bacillus of typhoid fever—though in the latter case death was produced with more pronounced general symptoms, and also more rapidly than in the former case.

While one mode of origin of the disease may be brought about in the manner indicated by Rodet and Roux (that is, in a manner only too likely to be ascribed to a spread of the disease by water-borne contagia), it is well known that another quite different mode of origin of the disease was advocated by Murchison in his celebrated work on "Continued Fevers."[1] He believed that the toxic cause of typhoid fever might originate in pent-up decomposing fæcal matter, and that in these cases the mode of entry of the poison into the system was through the air rather than by means of fluids taken into the alimentary canal.

Others, again, hold views closely related to this. Instead of supposing that the general health is lowered and the system poisoned by breathing the emanations from choked drains and cess-pits (causes to which isolated or small groups of cases of typhoid fever often seem traceable), they lay stress upon widespread pollutions of the soil beneath houses, and upon variations in the height of ground water of such a kind as to facilitate the entry of emanations from such soil into houses, and the production thereby of slowly poisonous effects upon many persons simultaneously. They would thus account for the endemic and epidemic visitations of typhoid fever in particular towns, and for their special autumnal prevalence. Speaking on this latter subject in a lecture on "Some Points in the Etiology of Typhoid Fever," Sir Charles Cameron,[2] the medical officer of health of Dublin, said: "Localised outbreaks of typhoid fever can frequently be directly traced to the use of a particular supply of polluted water or milk, but the widespread epidemics of this disease, and even its persistent occurrence in so many towns, must be due to some other cause or causes. For example, in Dublin it was epidemic in 1891–92, and in 1889 it appeared in all parts of the city and adjacent districts." These epidemics, Sir Charles Cameron feels assured, could not be traced to contamination of the water-supply, for in his view "there are few cities in the world with such good water as Dublin fortunately possesses." On the other hand, there

[1] "The Continued Fevers of Great Britain," 1862, pp. 437–456.

[2] "The Lancet," June 11, 1892, p. 1285.

is very poor natural drainage owing to the low-lying situation of the city, and the soil in very many parts is foul and saturated with decomposing organic fluids. The main cause in Dublin and in other cities of these epidemics of typhoid fever is, Sir Charles Cameron thinks, to be found in these very impure states of the soil. He says he has carefully investigated the subject and is "convinced that typhoid fever is often caused by underground air entering our dwellings" from such polluted soils.

Some years since I received a letter from Dr. Angus Mackintosh,[1] then medical officer of health of Chesterfield, in which, as a result of his experience, he professed his strong belief in the *de novo* origin of typhoid fever. He said: "If not, how can those who believe otherwise explain the mystery that enteric fever decreases in proportion as the sanitary condition of any district is improved, and that as a direct consequence and in every case . . . I say, from a lengthened experience in one of the most fever-stricken districts in England, and after carefully investigating 500 cases of that disease, in my capacity of medical officer of health, that 90 per cent. of these could not be traced by me or anybody else to a previous enteric case. The sanitary authority for which I act have borrowed £100,000 from the Loan Commissioners during the last four years for drainage works and water-supply, and by alterations and arrangements in regard to these important items they have reduced enteric fever already in the district to a very small proportion indeed."

The moral would seem to be that purity of soil is almost as important as purity of air or water; that bad drainage may pollute both air and water; and that some cases of non-specific fæcal contamination of the latter—and not only pollution by typhoid dejections—may act as causes of typhoid fever. I feel assured that these are safer and sounder doctrines than the narrower views promulgated by ultra-contagionists.

If we look to another disease or rather group of morbid conditions—namely, *Pulmonary Phthisis and Tuberculous Affections of other organs and parts*—a group so common and fatal as to constitute one of the scourges of the human race—there is room for the same uncertainty as to the proportional limits between their *de novo* origin and their spread by means of contagion,

[1] Letter dated August 26, 1876.

though the tendency of late has been to believe in contagion only. The great change of view that has been generally adopted by the medical profession in regard to this matter within the last few years is most remarkable and unprecedented. This change, too, has developed, not so much by reason of fresh and conclusive evidence as to the frequency of contagion in the human subject, but almost entirely from theoretical considerations and from unwillingness to believe that a disease caused by a Bacillus, and capable of being freely propagated experimentally among lower animals inoculated therewith, can also arise *de novo.* At the present time it is regarded as quite heretical to think it possible that phthisis can arise independently; while as late as 1896 we find Crookshank in his "Bacteriology and Infective Diseases" (p. 387) writing as follows: "Whether the disease in man is contagious is an open question, though numerous cases of supposed communication between husband and wife, brothers and sisters, have been reported, and Ransome showed that tubercle Bacilli were present in the breath in phthisis. On the other hand, the experience in consumption hospitals does not support this view, there being no evidence of the communication of the disease to nurses and hospital attendants." Such, then, have been the remarkable discrepancies in the common view entertained upon this question within a brief period of less than ten years.

At present there is a beneficent enthusiasm for "sanatoriums" in this and other European countries for the relief or the cure of patients who are afflicted with this common and very fatal affection. Phthisis is unquestionably capable of being mitigated —and even cured in many cases where it is not too far advanced— by plenty of fresh air and the best hygienic conditions.[1] This seems to me rather to lend favour to the view that it is commonly an affection produced *de novo* and altogether apart from contagion, as we formerly believed. If good hygienic conditions and improved vitality will lead to the cure of the disease, then

[1] Sometimes, too, even a simple surgical operation, such as drainage of the abdomen in a case of tuberculous peritonitis, will lead to a cure of the patient, notwithstanding the presence in his tissues of untold legions of the specific Bacilli. In his admirable address to the British Medical Association at its recent meeting Mr. Mayo Robson says ("British Medical Journal," August 1, 1903, p. 245): "I have seen patients reduced to the last extremity of weakness, where the mesentery was standing stiff with tubercle and the abdomen was swollen to an enormous size, recover completely and be thoroughly restored to perfect health from a condition apparently completely hopeless."

low vitality and bad hygienic conditions may have sufficed to produce it. But, it will be said, you forget the presence of the tubercle Bacillus. To which I would reply. Have not the experiments made for the artificial production of "Pasteur's septicæmia" almost completely got over this difficulty? The injection of a small quantity of a germ-free chemical irritant into the subcutaneous tissue of a healthy rabbit has made it plain that pathogenic microorganisms may either be produced by heterogenesis in the focus of inflammation thus caused, or else that the germs of common Bacilli existing in the healthy animal on which the experiment has been made have been roused, rendered extremely virulent, and have been converted, in fact, into pathogenic Bacilli, henceforth capable of acting as contagia for the indefinite propagation of this form of septicæmia. Here we have had, over and over again, in the plainest way, the *de novo* production of a contagious disease in which, as in phthisis, Bacilli act as the contagia. Why, then, should not an analogous process be similarly possible in the case of phthisis and other tuberculous affections? A Bacillus just as specific in its characters as the Bacillus tuberculosis makes its appearance also when "Davaine's septicæmia" is produced experimentally.[1]

Half a century ago, and less, many conditions now termed "tuberculous" were then spoken of as scrofulous; and scrofula was recognised as a condition of low vitality in which inflammations of skin and mucous membranes were common, in association with enlargements of lymphatic glands in the neck and axillæ, and

[1] In a recent Address delivered by Prof. Adami of Montreal ("British Medical Journal," May 27, 1905, p. 1135), he cites a very distinct case in which certain common Bacteria under the influence of altered conditions of life have had their metabolic processes completely altered so that, as he says, "From having been perfectly harmless they are now pathogenic and can set up disease." He then makes the following remarks: "What is to be said concerning the tubercle bacillus in this connection? In the first place we may have the complete assurance that Adam was not created suffering from tuberculosis. The bacillus we may be fairly sure, from living it may be on food-stuffs outside the body, accustomed itself first to living on the surface and in the passages of the organism as a harmless saprophyte, and only later gained the power of living not on but in the tissues, and from that moment it became pathogenic." He goes on to say that this "must have happened centuries and centuries ago," seeing that the disease was well known to early Greek writers on medicine. But all that we know of Bacteria would lead us to believe that days, or at most a week or two, would suffice for the passage from the non-pathogenic to the pathogenic mode of life; and what took place in the days of the early Greeks, and before their time, may be taking place now.

not infrequently, chronic diseases of the joints. Now, discarding the old term, it is the custom to speak of tuberculous joints and tuberculous lymphatic glands, because it is known that the specific Bacillus of tubercle is to be found in the tissues affected, though perhaps nowhere else in the body.

But how, it may be asked, in accordance with present ultra-contagionist views as to the mode by which tuberculous affections are disseminated, are we to explain the isolated occurrence of the tubercle Bacillus within the tissues of joints, in the lymphatic glands of the axillæ, or within some of those in the neck? The two generally admitted channels for the entry of such micro-organisms into the system are through the mucous membranes of the air passages and that of the alimentary canal; but entry by either of these routes would not satisfactorily account for their isolated presence in the glands of the axillæ, or in some of those not infrequently involved in the neck, or within the tissues of the knee or hip-joints.[1]

It has for a long time seemed to me that chronic inflammations of lymphatic glands in certain cachetic states of the system (such as we formerly labelled "scrofulous") may be of such a nature as necessarily to produce therein the little nodules which we recognise and name "tubercle." And this same view was very ably set forth in some detail by Mr. (now Sir) Frederick Treves,[2] at the International Medical Congress of 1881, in a communication entitled "Tubercle: its Histological Characters and its Relation to the Inflammatory Process, as shown in Tuberculosis of Lymphatic Glands." He pointed out that all the characters of the nodule known as "tubercle," apart from the Bacillus which had not then

[1] The frequency of tubercle Bacilli in association with pleurisy occurring in persons, previously to all appearances perfectly healthy, as a sequence of falls or blows upon the chest, and other facts cited by Prof. Osler in a valuable communication made to the British Medical Association last year, are difficult to account for from the point of view of infection ("British Medical Journal," Oct. 15, 1904, p. 999); and the difficulty increases in many cases of tubercular affections of the Fallopian tubes, seeing that G. L. Basso has shown by an experimental investigation on rabbits, that tubercular affection of the female genitals is of a descending rather than of an ascending type. But "tubal tuberculosis has been found to be commoner than that of the body of the uterus, while both are much commoner than vaginal, cervical, and ovarian tuberculosis"; and as infection from the peritoneum seemed to be excluded, the frequency of this affection is at present quite unaccounted for as a result of infection ("British Medical Journal," May 20, 1905, p. 1107).

[2] "Transactions of the International Medical Congress," vol. i., pp. 298–303.

been discovered, can be referred to a chronic inflammatory process. He said: "The so-called tuberculous process in the external lymphatic glands can often be traced to some simple inflammatory process that implicates the radicles of the gland. Here the first change communicated to the gland is essentially inflammatory and as that change develops in the organ it begins to assume peculiar features." Of course, it would now be said, Yes, what you say may be true, but the irritative or inflammatory influence, the effects of which you recognise, is really due to, and caused by, the presence of the tubercle Bacillus. To this it may fairly be rejoined that such a view ignores the real and often obvious cause—the irritation of the gland; that it ignores the general state of the system which pre-exists and co-operates; and that it postulates infection without proof as the initial cause of the whole phenomena. But in the absence of any rational or even plausible means of accounting, in accordance with contagionist theories, for the presence of tubercle bacilli in certain lymphatic glands and in these alone, or in some joint and nowhere else in the body, it is, I think, most in harmony with existing knowledge to suppose that this particular Bacillus is a product rather than a cause of certain inflammations occurring in lowly vitalised subjects, in just the same way that the appearance of the Bacillus of "Pasteur's septicæmia" seems, as Burdon Sanderson put it, dependent "on the intensity of the inflammation itself." The adoption of such a view would go far to explain many difficulties; and the question of the etiology of tuberculous affections would be greatly simplified and brought again more closely into accord with former views.[1]

Quite recently, however, von Behring has been advocating a very different hypothesis.[2] He believes that infection takes place in the main in infancy, through the intestinal canal, and that thereafter the infecting Bacilli lodging in different parts of the body commonly remain latent for years, perhaps for a long series of them. He relies in part upon recent investigations showing the high percentage of cases in which tuberculous lesions of some kind are to be found at necropsies, or demonstrated during life by tuberculin and other means; and in part upon his own researches demonstrating that tubercle Bacilli can, in infancy more especially, easily pass from the intestine into the lacteals and thence into the

[1] See "Brit. Med. Jrnl.," Aug. 19, 1905, p. 388, concerning the occurrence of the Bacillus of tubercle in lymphatic glands.

[2] "Deut. Medicin. Wochen.," Sept. 24, 1903.

blood. He discredits the now commonly accepted views as to the frequency of infection through the lungs. It seems true that he has proved the possibility of infection with tubercle or other Bacilli in the manner he indicates. I, on the other hand, have proved the possibility of their *de novo* origin. He postulates long periods of latency after systemic infection, hard to be believed; and ultimately requires agencies for waking the tubercle Bacilli into activity, just such as I suppose may be adequate for calling them into being—namely, malnutrition and conditions of lowered vitality, howsoever produced, though among such factors impure air and inadequate or improper food must take an important place.

It would, of course, be an easy step to recognise the extreme probability that the conditions which had sufficed for the appearance of the characteristic Bacilli in the glands and in the joints might also obtain in the lungs. We might then return to something more like the sober views that prevailed concerning the etiology of phthisis only a few years ago, when the affection was freely recognised as generable in the individual, altogether apart from contagion, and contagion was supposed to take only a limited share in the production of the disease. This seems the more rational and most warranted view to take. It is one which would tend to lay stress upon the need for prevention as well as cure, but it would not encourage the view that the disease could be exterminated, or even very largely diminished, by the provision of "sanatoriums" and by efforts to minimise the risk of contagion. I merely mean to imply that, in my opinion, contagion is as much over-rated as genesis is under-rated, and that our notions concerning prevention must not be too much centred upon the mere elimination of contagion.

I will only briefly refer to one more disease, but to one having several points of agreement with the affections last considered and concerning which much discussion has been taking place of late. I allude to that terrible affection *Leprosy*, which is generally admitted to be contagious only in a low degree, and under the influence of very special conditions. Being an affection so slightly contagious and yet so widespread in different countries, the question naturally arises, Is it not also generable *de novo?* It was in the past freely believed to be so, but since 1874, when Henson discovered that a Bacillus was always to be found in the tissues affected, there has been a growing antagonism to this view, owing to the yearly increasing importance of bacteriological work and to the ultra-

contagionist doctrines that bacteriologists seem invariably to favour.

Formerly the malady was commonly ascribed to the conjoint influence of bad hygienic surroundings, poverty, and exposure, together with deficient and improper food, more or less putrid. Of these conditions, it would seem quite possible that some peculiarities in food may have been, and may still be, most potential in favouring the development of the disease. It seems to me that Mr. Jonathan Hutchinson has distinctly strengthened this view, and brought forward some valuable evidence in support of his own position that badly preserved and semi-putrid fish is one of the most important factors pertaining to this category. At all events, whether his conclusions are to be accepted as correct or not, his personal researches in South Africa and in India in collecting evidence on this very important subject, involving as they must have done so great an expenditure of time and labour, are surely worthy of the highest praise.[1]

The conclusion to which he has come in regard to the relative frequency of the spread of the disease by contagion and its *de novo* origin is very similar to that arrived at by the Leprosy Commissioners in India. Their verdict, given in 1890–91, was that the influence of contagion was "as small as, or even rather less than, in the case of tuberculosis,"[2] and that in the great majority of cases the disease originates *de novo*. This I regard as a perfectly logical position, and one quite explicable in accordance with known facts. But it would be absolutely repudiated by many, perhaps by most, ultra-contagionists, as may be gathered from an article on Leprosy in the "Quarterly Review" for April, 1903, from the pen of Dr. Geo. Pernet. Referring to this verdict of the Commissioners in India, and reflecting perhaps the prevailing medical opinion of to-day, this writer says (p. 397): "But did any one of the Commissioners or does any one with knowledge of the subject contend that tuberculosis arises *de novo?* What induced the Commissioners to come to the conclusion as to the *de novo* origin of leprosy is a psychological puzzle, and it is difficult to see how the supporters of such an hypothesis account for the presence of Bacilli in the leprous

[1] An account of his investigations will be found in the "Transactions of the Royal Medical and Chirurgical Society," 1902, p. 161; and in "The Lancet," May 9 (p. 1316), 23 (p. 1465), and 30 (p. 1938), 1903.

[2] At that time the influence of contagion was believed to be very slight in tuberculosis.

patient. Are we to suppose that these microorganisms arise spontaneously?"

In the debate which took place at the Royal Medical and Chirurgical Society, Mr. Hutchinson met with the same kind of intolerance, and there was a similar ignoring of facts which would have permitted his critics to find less difficulty in accounting for the presence of the Bacillus in a case in which leprosy had originated *de novo.* They should not have forgotten the specific Bacillus of "Pasteur's septicæmia," which can be produced at will by skilful investigators with the aid not of semi-putrid food but of a few drops of putrid blood. It was not even absolutely necessary, as I have shown in this communication, that they should believe in a *de novo* origin of the Bacillus itself; it was still less necessary that they should require its presence to be demonstrated in the bad fish or other food; and it was absurd, as I take it, gravely to attempt to shunt the real question by the gratuitous statement that "the origin of the germs of disease was probably in the remote geological past." It is a pity the able author of this sentiment did not give us some hint as to the mode of production of these germs in the past, and tell us what his warrant was for using the word "probably" in such a connection.

What I have said in regard to the possible *de novo* origin of tuberculosis would, in fact, with slight variations be applicable in regard to leprosy. It cannot be denied that the disease is to a slight extent contagious, but there is much evidence to show that in the main it arises *de novo.* There is certainly nothing unreasonable in the supposition that putrid fish, or other bad food of like kind, may carry into the system Bacteria and the toxic products which they have formed, and that the continued influence of such bodies may, in some persons, act as irritants, and either engender or awaken organisms in this or that tissue having the characteristics of the leprosy Bacillus—just as the boiled dilute liquor ammoniæ injected into the subcutaneous tissue of a guinea-pig or a rabbit produces, even within a few hours, swarms of the Bacillus met with in "Pasteur's septicæmia," or as two or three drops of putrid blood in the same situation may give rise to the appearance throughout the body of swarms of the more distinctly specific Bacilli which suffice for the indefinite propagation of "Davaine's septicæmia."

In any case the importance of diligently seeking after the cause of leprosy must be admitted, and Mr. Hutchinson is obviously

right when he says[1]: "It cannot be necessary to insist that the prevention of leprosy is a work of far greater beneficence than is the mere provision for the care and comfort of the leper. . . . Not only in India but in South Africa, the West Indies, and many other of our colonies, the saving in money as well as the mitigation of human suffering would be immense if the leprosy question were once settled. Large sums are now benevolently devoted to asylums for lepers. My conviction is strong that one-tenth of the sums thus annually expended would, if devoted to discovery of cause, render these establishments unnecessary and save their cost for all time."[2]

What has happened in regard to typhus fever affords the strongest testimony as to the value of the broader outlook—the search, that is, for the conditions of origin of a disease. The ravages of typhus in our crowded cities and in our jails has been enormously curtailed—not so much because of its diminished spread by contagion, but because we have learned what are the causes which engender it, and are, therefore, better able to prevent its occurrence. There can, moreover, be little doubt that no impassable barrier exists between non-pathogenic and pathogenic Bacteria. The mutability in form, and changeability in activity, of all these microorganisms is immense. They may merge into one another, just as the clinical types of disease with which they are associated may be united by almost insensible transitions. In a recent able communication on the "Borderlands of Diphtheria and Scarlet Fever," we find Dr. Biss, after a large experience in a fever hospital, saying,[3] "the *nuances* between these conditions—scarlet fever, diphtheria, and tonsillitis—are so gentle that each shades off into the other not at one but at many points." Of course, if this is true, it can only mean that there are similarly minute transitions between the activities of the microorganisms associated with the maladies in question; and that there must be the production, under certain circumstances, of "specific" from common microorganisms habitually present in the parts affected.

This would again help to bring us very much to the point of view long ago advocated by the late Professor Hueter who said at

[1] "The Times," May 25, 1903.

[2] The following paragraphs are additions made to this article since it appeared in the columns of "The Lancet."

[3] "The Lancet," Nov. 7, 1903, p. 1291.

the International Medical Congress held in London in 1881: "Although it is impossible not to recognise the specific modes of activity of microorganisms in the production of infective diseases, we need not on that account deny that there is a certain unity in all these microorganisms. I am of opinion that this unity is founded upon the processes of putrefaction, and that the specific modes of activity must be regarded as depending upon certain alterations in the putrefactive process." It cannot be said that the vast mass of subsequent investigations have displaced such a view, seeing that no less an authority than Prof. Hueppe, of Prague, in his first "Harben Lecture," recently delivered in this city, is reported to have said existing evidence favoured the view that the origin of all common infectious diseases was "phylogenetically traceable to putrefactive processes."[1]

Let us then strive to ascertain the conditions of origin of all contagious affections. The more contagious they are, the more important does the quest become. Let us not blindly think that contagion is the one and only cause, but seek in all doubtful and obscure cases, and by cumulation of evidence, to ascertain what are the invariable and immediately antecedent sets of conditions, or states of system, that may have sufficed to engender this or that contagious disease. Progress, however slow, may in this way ultimately reward our efforts, and we may gradually attain a knowledge that will confer great power in checking the ravages of these pestilential affections—a power to which we shall never attain so long as we pin our faith exclusively to the narrower ultra-contagionist doctrines now so prevalent.

[1] "The Lancet," Oct. 31, 1903, p. 1217.

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